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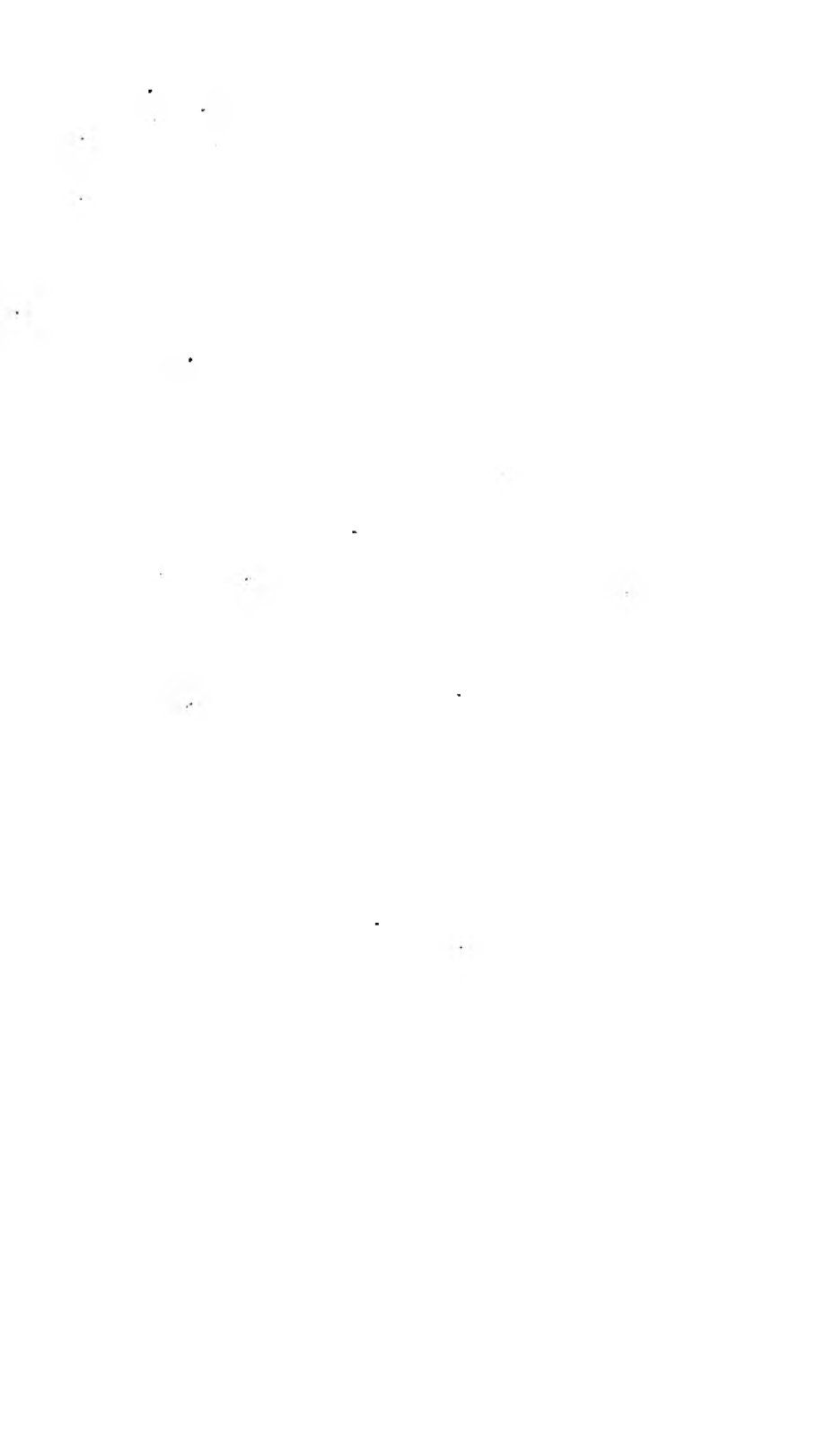
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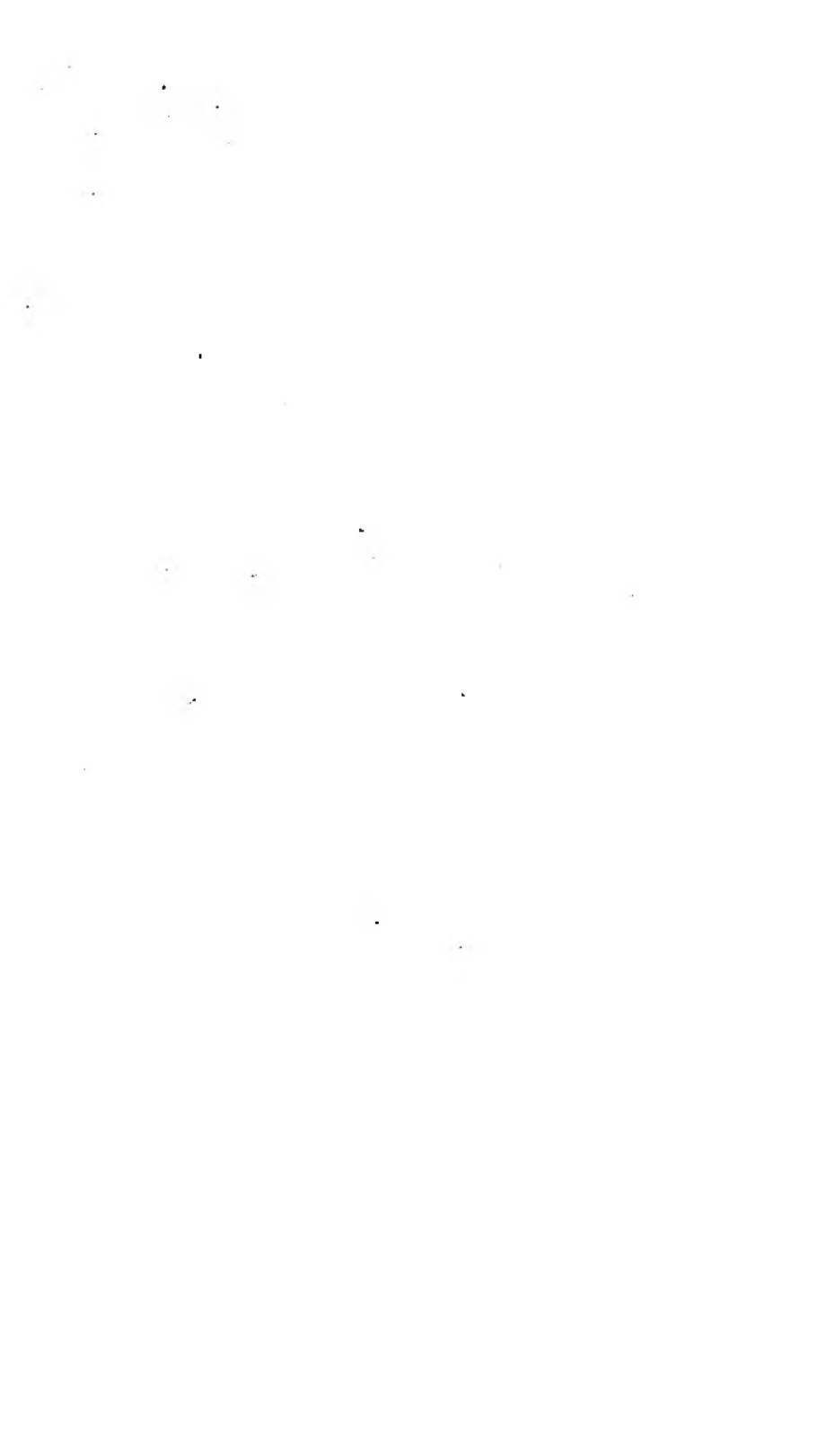


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**JOURNAL**

OF THE

**SOCIETY OF TELEGRAPH-ENGINEERS  
AND ELECTRICIANS.**

FOUNDED 1871. INCORPORATED 1883.

INCLUDING

**ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND  
ELECTRICAL SCIENCE.**

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The One Hundred and Sixtieth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 13th, 1887—Professor D. E. HUGHES, F.R.S., late President, in the chair.

The minutes of the Annual General Meeting were read and confirmed.

The names of new candidates were announced, and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Edwin Blakey.

Gisbert Kapp.

W. Lant Carpenter.

Alfred Thompson.

From the class of Students to that of Associates—

David Abercrombie.

F. W. A. Knight.

G. H. Bailey.

William Taylor.

George E. Fletcher.

E. J. Wade.

Professor HUGHES: It is my sad duty to-night to announce the death of one of our most esteemed members, Colonel Sir Francis Bolton. It is a loss which we all very greatly deplore, and the Council feel that this occasion could not be allowed to pass without moving a resolution to be sent to Lady Bolton. I am sure all present will wish to associate with us in this message, which is as follows:—"That the President, Council, and Members of the Society of Telegraph-Engineers and Electricians desire to record the deep regret occasioned to them by the death of Colonel Sir Francis Bolton, Vice-President and Honorary Secretary, who was one of the founders of the Society, and to whose energy and liberality much of its early success was due; and they further desire to express their sincere sympathy with Lady Bolton in her bereavement." Before the motion is put, I will ask Major-General Webber to make a few observations.

Major-General C. E. WEBBER, C.B.: We all must regret very much, at this our opening meeting of the year 1887, that it is our sorrowful necessity to deplore the loss of one who has been amongst us for so many years. I hope the meeting will excuse me if I occupy a minute or two in mentioning a few facts in reference to the career of Colonel Sir Francis Bolton, and especially in connection with this Society.

For twenty years I have known him as a friend, and at the time I first made his acquaintance he had already distinguished himself in the Army, and had been appointed Assistant Quartermaster-General, serving at Chatham, having been brought there for the purpose of instructing the officers of Royal Engineers in signalling. Selected for that work by that good old soldier Sir John Burgoyne, he filled the position with great credit, and introduced what were (although not new to the Army) systems which have been of material use in advancing the science of telegraphy in warfare, and which have been more or less in use ever since. Associated with Captain Colomb of the Royal Navy, he brought out, as we all know, a code book which was for many years the means of bringing the Army and Navy together for one object, viz., for communication by visual signalling. About 1869 he left the Army, and entered on industrial undertakings, which

he carried on at No. 4, The Sanctuary, where we have almost ever since the year 1872 been his tenants.

Early in the year 1870, I think it was in May, he and I brought our heads together one day on an idea which had come into my mind just after attending a meeting of the Association of Gas Engineers and Managers, and with his bright foresight he seized upon the suggestion of forming a Society for Telegraph Engineers. During that summer, we and Mr. Sabine (whose loss we have also had to deplore) met frequently, and at those meetings the birth of our Society took place. I must say that it was to Major Bolton (as he was then) that we owe our existence as a Society. It was due to his energy and to his far-seeing views of the position that telegraphy was going to take in this country, aided by the researches he had himself made in signalling, and which he had made so extremely interesting a subject. Although I have had the honour of my name being associated with him as one of the founders, I must say, and have always said, that if it had not been for Frank Bolton the Society might not have come into existence at the early period that it did. During late years we have not seen so much of his pleasant, happy face at this table as we did during the first four or five years of the Society, but he never had lost his interest in it; and when other tasks took him away and absorbed his time, he never, in his office, or at any other times when we met, forgot to say or do something to further and help the work of this Society. His life, as we all know from what he gave himself to do, was one of increasing toil. I have rarely met a man who could get through so much work in twelve hours, and it was in that work, in all directions, whether it was for the advancement of his occupations or for the advancement of science, that he toiled on in harness to the end. I was told, by those who were near him at his last moments, that on the morning of his death he asked to see, and saw, many of his letters. To such a man as that, as also one of our founders, we must all look with great respect, and his memory will dwell with us for many years. I think that the meeting will join cordially in agreeing that a letter of warm condolence shall be sent to Lady Bolton, expressing our deep regret and our

unanimous sorrow for the loss of our departed brother Frank Bolton.

Professor HUGHES: You have heard the motion and the eloquent words of Major-General Webber, which express so well the grief which we all feel. I am sure that you all wish to associate with us in this vote of condolence, and I ask you to unanimously pass it by holding up your hands.

The motion was carried unanimously.

We have now a most pleasant duty to perform, and one which gives me very great pleasure indeed, and that is to present the premiums which have been offered by our Society to the authors of those papers which, after examination by the Council, have been found to be worthy in every respect of this high mark of their approval. During the past year we have had most excellent papers by members of our Council, but these, according to the rules of our Society, were not eligible for the premiums; very fortunately, however, we had three meritorious papers by members and associates not on the Council, and each of these were found, upon examination, to possess such high merit as to fully entitle them to one of our premiums.

The "Society's Premium," value £10, has been awarded to Mr. Alexander Bernstein, for his paper entitled "Electric Lighting by means of Low-resistance Glow Lamps." In that paper he not only treated of his special form of lamp, but worked out every detail, from the dynamo to the lamp, thus constituting a distinct and very original system. I am sure that the results of the beautiful experiments he showed, and the character altogether of the paper, make it pre-eminently suited for our premium, and it is with great pleasure that I now hand Mr. Bernstein the microscope which he has chosen. The "Paris Electrical Exhibition Premium," value £5, has been awarded to Captain H. R. Sankey of the Royal Engineers, for his paper entitled "On a Problem relating to the Economical Electrolytic Deposition of Copper." The subject treated by Captain Sankey is one that has been too little dealt with. Electro-metallurgy has made enormous strides since the application of the dynamo, and Captain Sankey has dealt with the subject in a way that is appreciated greatly, not

only by the Society, but abroad; for Professor Moses G. Farmer, of Boston, U.S.A., who has had great experience in the electrolytic deposition of copper, has written me a letter on this subject, saying that he regards Captain Sankey's paper as one of very great importance. I must also congratulate Captain Sankey for having been able twice in succession to obtain one of our premiums. He has chosen a clock and Maxwell's "Treatise on Electricity and Magnetism," which I now present to him with very great pleasure. The "Fahie Premium," value £5, has been awarded to Mr. H. Kingsford for his paper, "On a Method of Localising a Fault in a Cable by testing from one end only." Mr. Kingsford is at present in Peru, and probably does not yet know that the premium has been awarded to him. The method employed by Mr. Kingsford seems to be original and of practical value: he has employed it himself in testing submarine cables, and no doubt its publication in our Journal will lead to its extended use. We particularly desire to have papers relative to progress in electric telegraphy, and we hope our members and associates will bring before the Society any serious improvement that has been made in this most important branch of applied science.

Another duty which I have to perform is to thank you, gentlemen, most sincerely for the very kind attention which you have given me during my presidential career. It is owing entirely to the kindness and forbearance on your part that I have been able to surmount the difficulties of my situation.

I have also another most agreeable duty, and that is to introduce my most worthy and able friend the President-elect, Sir Charles T. Bright, and ask him to take the presidential chair and read his presidential address, to which I am sure we shall all give the most earnest attention.

The President, Sir Charles T. Bright, then took the chair.

Professor G. FORBES: Before we proceed to the other business, there is a duty which I feel sure every one here will heartily sympathise with me in fulfilling. I wish to propose a hearty vote of thanks to our Past-President, Professor D. E. Hughes, for the able way in which he has filled the chair during the past year. It falls to our lot, in different years, to have Presidents

whose names are associated with different parts of our science. At one time we have a man whose name is associated with telegraphy; at another time one who represents physical science; and at another time, perhaps, one who has done great things in other practical applications of electricity. We have always felt that the name of Professor Hughes stood very high in the opinion of the world, not only as the inventor of one of the most important Printing Telegraph Instruments ever constructed—not only for his inventions connected with the Microphone, the Induction Balance, and other allied instruments, which I heard eulogised so much by Professor Stokes, on the presentation to Professor Hughes of the medal of the Royal Society—not only in those directions does Professor Hughes' name stand very high in the civilised world, but also, and from a point of view which I look at with especial interest—as an investigator of pure science—an investigator of the very rare and much to be desired kind, a pure lover of science who investigates science with the object of trying to find out new facts in nature. For these reasons, not only has Professor Hughes been a worthy President to us, but he has by his work, shed a lustre upon the Society over which he has presided. But apart from these qualifications, he has, as every one in this room must have observed, conducted his duties in a manner which showed his appreciation of the onerous post which he occupied; and he has devoted himself heartily during the past year to the interests of the Society. Whether in this room or in the council-room, he has evidently always had in his mind that he was in a responsible position, and that it was his bounden duty to devote himself to the interests of the Society. He has done this perseveringly during the whole time until this very day; and I have the greatest pleasure in proposing a most hearty vote of thanks to Professor Hughes for the way in which he has conducted the duties of President of the Society during the past year.

Mr. W. H. PREECE: I have very great pleasure in seconding this proposition. There are some here who know the difficulty we had in inducing Professor Hughes to accept the position of President, but we succeeded, almost against his will, and I do not



think there is a single man in this room who regrets the choice that we made.

The President put the motion, which was carried most heartily.

Professor D. E. HUGHES : My emotion is too great to find words to express adequately my thanks for the motion you have so cordially passed, nor to sufficiently thank Professor Forbes and Mr. Preece for the very warm words they have used in my regard. It was with the utmost diffidence that I accepted the high honour you kindly bestowed on me when you elected me your President, but I resolved to do all in my power to forward the interests of the Society. If I have succeeded, it is entirely due to the constant aid and energetic support of our Council. All who have been on our Council know how earnestly they work in the interests of the Society, and I have to thank them sincerely for the kind support with which they have endeavoured to render my task as light as possible. Further, I have to thank our most able and worthy Secretary, Mr. F. H. Webb. We all know how the success of a society depends upon an energetic and able secretary, and Mr. Webb has not only attended to all the duties of the Society, but he has done all in his power to lighten any labours which I might have, and therefore I desire also to give him my warmest thanks. Allow me in the these few words to thank you all for your great kindness.

#### ADDRESS OF THE PRESIDENT, SIR CHARLES BRIGHT.

In addressing you at the first meeting of our new session, I wish in the first place to express my most cordial thanks for the honour conferred upon me in being elected to the office of President during the present year, the more so because it is a period specially interesting to us, as in it occurs the Jubilee of the Accession of our beloved Sovereign, and also of the first practical realisation of the electric telegraph.

I am glad to be able to state that our Society continues to flourish; and to maintain its justly acquired reputation for the value and utility of the papers read, and of the discussions upon them; which are often of more importance than even the papers



themselves, as bringing forward views and information on different sides of the questions raised ; and, while I refer to this, I wish to say that I should like to hear more, if possible, during the discussions in the course of this session, from those who have not been so long in the Society as some of us, and the only way to obtain this result is that those who join in the early part of any discussion should make their remarks as concise and as much to the point as possible, so that our juniors may have time left to speak.

The total number of our members of all classes is now 1,343. At the first meeting, in February, 1872, it was only 110. At that meeting the President, referring to the *raison d'être* of the Society, adverted to the impossibility of one great scientific body like the Royal Society succeeding in cultivating all the different departments of science in detail ; and that therefore other societies, like the Astronomical, the Geological, the Chemical, or our own, were essential for their own especial fields of scientific knowledge and practice.

It is somewhat curious that Dr. Priestly had the same views when, so long as one hundred and twenty years ago, in the preface to his "History of Electricity," he suggested that an Electrical Society should be formed, to be devoted to electrical and kindred investigations. "The business of philosophy," he said, "is so multiplied, that all the books of general philosophical transactions cannot be purchased by many persons or read by any person. It is high time to subdivide the business, that every man may have an opportunity of seeing everything that relates to his own favourite pursuit, and all the various branches of philosophy would find their account in this amicable separation. Let the youngest daughter of the sciences set the example to the rest, and show that she thinks herself considerable enough to make her appearance in the world without the company of her sisters."

This suggestion of Dr. Priestly, made in the year 1767—long before galvanism, electro-magnetism, thermo-electricity, and magneto-electricity were known—bore no fruit, at all events not for many years.

In June, 1837, however, a society was formed entitled the "London Electrical Society," of which Mr. Gassiot and Mr. Sturgeon were the principal founders, one becoming, subsequently, the treasurer, the other the first president. The name of the former will be well remembered for his researches in electricity, and for the liberal manner in which he constructed apparatus on a large scale for the purpose. The latter name suggests a long record of experiments, of which the most valuable was his discovery of magnetising bars of soft iron and rapidly changing their polarity, by voltaic currents; in other words, the invention of soft iron electro-magnets, which are so largely used in telegraphs, telephones, and almost every kind of electrical appliance.\*

Mr. Sturgeon, the President of that Society, in his inaugural address in October, 1837, claimed "that electricity was the most important experimental science ever cultivated by man," and remarked that the preceding forty years had been more productive of electrical discovery than all the antecedent centuries embraced in the history of the science."†

Sturgeon was justified in his conclusion: for those forty years included the labours and discoveries of Volta, Brewster, Arago, Humboldt, Wollaston, Davy, Oersted, Ampère, Schweigger, Ohm, Becquerel, and, above all, of Faraday, who was then in the midst of his ever memorable experiments.

A number of papers, upon almost every branch of electrical science then known, were contributed to the proceedings of the London Electrical Society; part of which were published in its *Transactions*, which will be found in our library, and the remainder in Sturgeon's "Annals of Electricity."

The Society, however, lacked in vitality; there were in its best days only 76 members, and it was finally dissolved in a little less than six years.

The late Mr. C. V. Walker, who was one of its members, having joined in April, 1838, told us, in his presidential address here, all about the decline and fall of that small society. He ultimately

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\* *Trans. Soc. of Arts*, vol. xliii., 1825.

† "Annals of Electricity," vol. ii., p. 64.

became both treasurer and secretary. As treasurer, in the culminating year of decay, he only received £77 with which to pay himself as secretary, for the printing and publishing of the proceedings and all other expenses of the Society.

Now our Society of Telegraph-Engineers and Electricians commenced its yearly accounts at the end of 1872 with subscriptions to the amount of £422. In its sixth year the treasurer received £1,275, of which £615 was expended in printing and issuing the Society's valuable journal. The income last year amounted to £1,818.

The income of the Society was, during the first years of its existence, almost exclusively derived, as it still is to a very large extent, from the subscriptions of those who are more or less intimately associated with electric telegraphs, for, although every other branch of applied electrical science is now well represented on the list of members, a large contingent of subscriptions still come from those engaged in the Postal Telegraphs, in the Indian Government Telegraphs, in the large submarine cable companies, and, in fact, from telegraphists spread over the face of the globe; others, again, from the large manufactories connected with the supply of telegraphic instruments, materials, cables, and their accessories. Our Society might well use the motto of the Royal Engineers, "*Ubique*," or, let us say, "*Quæ regio in terris nostri non plena laboris*."

A purely electrical society might perhaps have had more success than its predecessor, at the time when our Society was established, but I doubt it; for, on investigating the practical applications of electricity for the general use of mankind during the last half-century, we find that nearly the whole of the work done and capital invested has been connected with electric telegraphs, at all events until 1878, since when a material movement has taken place in the development of electric lighting and telephones.

The earlier part of the present century was one of surpassing interest in the advance of electrical knowledge, and all the requirements for an electric telegraph were at hand: the voltaic battery, the electro-magnet, the multiplying coil, the magnetic

needle, together with the knowledge that, if suspended, it could be deflected by a galvanic current passing through a fixed coil adjacent to it; all were ready, and it is not to be wondered at that numerous devices for carrying on telegraphic communication by means of electricity were proposed and shown by many philosophers and experimentalists in different parts of the civilised world.

Ampère himself proposed, on the suggestion of the illustrious La Place,\* that a telegraph might be made with needles deflected in such a manner as to communicate different letters of the alphabet; and it was subsequently computed, at the trial of an American patent case, that more than sixty claims might be made out for suggestions of various kinds for an electric telegraph prior to its actual realisation.

It is no part of my purpose to attempt to award the proportion of merit due to each or any of the long array of inventors. Hundreds of most promising discoveries have died an early death for lack of industry or perseverance to foster them. The man who begins by inventing, and afterwards struggles through every obstacle and with the greatest difficulty brings it into actual practice, outstrips, to my mind, him who is merely the *projector* of even the most ingenious invention which history records.

A man of genius and perseverance, such as I have pictured, thus expressed himself some years later upon the subject:—

“If the electric telegraph were to be described generally in a few words, how should it be described? Might it not be called an application of a few known principles by means of a few simple contrivances to produce a practical result, which the experiments of scientific men, although their attention had been directed to the subject for a long series of years, had failed to produce? The merits of the invention must therefore consist, to a very great degree at least, in the practical realisation of that which before had been an idea or an experiment.”†

The writer of the foregoing was Mr., afterwards Sir William,

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\* “*Annales de Chimie*,” I. xv., p. 72.

† See “*Cooke's Comments on De Hamel's book*,” p. 70.

Cooke, an officer in the Madras Army, who returned from India on furlough in 1831.

The Liverpool and Manchester Railway had been opened for public traffic a few months before, and had proved a great success, the receipts being what was then considered the enormous sum of £250 a day, or a little more than £90,000 per annum. This was derived from fares of 7s. 6d., and three trains running each way daily.

The public were fully appreciative of the boundless advantages of this stupendous power, and the general interest in it was almost without parallel.

Notions of speed and distance were still relative, but their meaning had been changed.

New railways were announced from London to Birmingham, thence to Manchester, and in many other directions. Cooke was a man of great intelligence and scientific tastes, and the effect on the history of telegraphs produced by the interest which this altered state of locomotion awakened in his mind will be seen hereafter.

A few years later, viz., in March, 1836, when at Heidelberg, he saw for the first time, at the lecture-room of the Professor of Natural Philosophy, one of those experiments to which I have referred as being frequently exhibited to illustrate the possibility of telegraphing to a distance by electricity; it was fitted up between the professor's study and the lecture-room, and consisted of a pair of suspended needles, and fixed coils much after the fashion of Ampere's idea suggested by La Place fifteen years before, which had, however, like many others, been unproductive of any useful result.

Cooke was deeply impressed by this experiment, and with the conviction that electricity might be applied as an instantaneous means of communication for the working of the railway system then extending all over England, as well as for Governmental and general purposes.

So sanguine was he as to the success of his scheme that he at once abandoned his former pursuits and devoted himself exclusively to the practical realisation of an electric telegraph. Within

three weeks he made his first telegraph, besides working out numerous supplementary details.

In November, 1836, he showed to Mr. Faraday the apparatus which he had constructed in order to exhibit to the directors of the Liverpool and Manchester line.

No grass had grown under his feet since the idea had flashed on his mind some months before at Heidelberg. He was still absorbed in the first notion which struck him, of associating his electric telegraph with the working of railways for their mutual "safety and economy."\*

Early in the next year he became acquainted with Professor Wheatstone, who had been in the habit of showing, in his lecture-room, the feasibility of telegraphing by electricity, using two galvanometers and a permutating key-board by which deflections of the magnetic needles could be exhibited.

There is no occasion here, or in any civilised country, to descant upon the great scientific attainments and achievements of Sir Charles Wheatstone, from his memorable experimental determination of the velocity of electricity, in 1834, to the remarkable recording telegraph apparatus perfected by him in later days with the skilful aid of our ingenious mechanical engineer, Mr. Stroh, a member of our Council.

The result of the two experimentalists becoming known to each other was that they soon after agreed to combine their inventions; a patent in their joint names was applied for, receiving the Great Seal on the 12th June, 1837.

The specification of this, the first patent for electric telegraphs in any country, is very elaborately drawn up, occupying forty-six large printed pages and three large sets of drawings showing the details of their inventions. It comprises a complete reciprocal telegraphic system: indicating instruments of several kinds, sending and receiving keys very much like some of those used even now, methods of supporting and insulating the conducting wires, alarms worked by relays, and means of ascertaining faults in the line-wire by the use of detectors. The wires were

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\* "Telegraphic Railways." By W. F. Cooke.



to be placed in troughs, being previously covered with cotton and a resinous cement, and with varnish of different colours for the several wires so as to distinguish them in case of repairs being needed.

It will be seen by the foregoing abstract, and still more by any one who will examine this highly interesting specification, that the joint patentees had considered in a most careful manner many of the requisites and contingencies of the work which they were about to undertake. I may add that in after years the validity of the patent was upheld in two cases of infringement. Soon after the patent was granted, permission was given by the directors of the London and Birmingham Railway to lay down the wires between Euston Square and Camden Town Station; and by the latter part of July, 1837, the first practical realisation of the electric telegraph in its application to railway working was ready for trial.

Late in the evening of the 25th of that month, Mr. Cooke and Professor Wheatstone stationed themselves—the one at Camden Town Station, the other at Euston Square. In order to try whether the instruments would work through considerable distances, some miles of wire along which the current had to pass (besides the wires in the open air) were suspended in the large carriage-house near the Euston Square terminus, making the length 19 miles. Several friends of the inventors were present at Camden Town, among others Mr. Brunel and Mr. Stephenson. Professor Wheatstone first spelt out a message, and, on Mr. Cooke quickly and clearly answering from Camden Town, the practical realisation was accomplished. “Never did I feel such a tumultuous sensation before,” said the Professor, “as, when all alone in the still room, I heard the needles click, and as I spelled the words I felt all the magnitude of the invention now proved to be practical beyond cavil or dispute.”\*

I myself well remember experiencing feelings somewhat akin to those of the Professor some eight and twenty years ago, when on board the “*Agamemnon*,” steaming slowly into Valentia Bay,

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\* *Quarterly Review*, No. clxxxix, p. 125.

finishing the laying of a telegraph cable two thousand miles long, which extended to Trinity Bay, Newfoundland, the greater part being under water two miles in depth.

The apparatus used on that ever memorable occasion at Euston and Camden Town was the diamond-shaped dial instrument, with vertical needles on horizontal axes, described in the specification and shown in the drawings. The instrument required five wires, and it may be asked why should instruments like this have been employed when they had others needing but one? The reason was, that the former called for no skill in sending and receiving the messages, each letter being expressed by a simple signal of two of the needles converging to a letter. With this the inventors could easily telegraph to each other, and railway *employés* could take up the work after them without any delay, but I doubt if either Cooke or Wheatstone could have spared the time at that period to become proficient in sending or receiving a telegraphic code. The wires, which were laid up in a rope and placed in a trough, did not cost much in the manner they were made, though good enough for the time and the short distance; moreover, the battery-power required was small, and its action in deflecting the vertical needles to one or the other side was very simple and certain.

Thenceforward the electric telegraph prospered without a check. Wires were laid down on the London and Blackwall, and on the Great Western line between Paddington and Slough, and elsewhere. At first it was used only for railway purposes, but afterwards despatches were transmitted for the public at 1s. a message (without reference to length), which was the first popular use of the telegraph in England or any other country. Its application soon became nearly as miscellaneous as now, affecting the highest and lowest in the land—now acquainting the Queen that Prince Albert was leaving Paddington for Windsor, now effecting the capture of thieves going down for business on an “Eton Montem Day;” at one time sending the Queen’s speech at Westminster for the benefit of the Royal borough, at another ordering whitebait from London, or enquiring about luggage left behind.



All at once the country was awakened to the importance of this new means of communication by the result of the following message from Slough to London :—

“A murder has just been committed at Salthill, and the suspected murderer was seen to take a first-class ticket for London by the train which left at 7.42 p.m. He is in the garb of a Quaker, with a brown greatcoat on which reaches nearly to his feet. He is in the last compartment of the 2nd first-class carriage.”

A little difficulty arose in transmitting this message, for in the signals of the instruments v answered for u, and there was no q, so the word “Quaker” was spelt KWAKER, which the operator at Paddington did not at first comprehend. However, the delay was not enough to prevent proper arrangements being made for Tawell's reception.

On arriving at the Paddington Station, after mixing with the crowd for a short time, he got into an omnibus, the conductor of which was a policeman in plain clothes. Tawell, the Quaker, no doubt thought, as one passenger got in and another was put down, that his identity was getting better mixed each time. At last, reaching the Bank, he got out, paid his fare, and after crossing and recrossing London Bridge, and making many turns and doubles, he went to a lodging-house in Scott's Yard, Cannon Street. He had scarcely entered the hall when the omnibus conductor opened the door, and asked him -

“Haven't you just come from Slough?” “No,” said he. He was of course arrested at once, and afterwards tried, found guilty, and executed.

The effect of Tawell's capture was a greatly increased demand for the telegraph, and a great extension of the system.

Cooke, who, in his deed of partnership with Wheatstone in 1837, had reserved to himself the exclusive management of the invention and the sole control of the engineering department, found his labours prodigiously increased. To use the words of one who was with him at the time: “With his own eye and his own hand he directed all the operations in the actual erection of the first telegraphs; he literally lived, for the time being, upon the

railway, making a railway carriage his shelter by day and his couch at night.”\*

He still held fast to his original plan of allying the telegraph with the railway, which afforded a way-leave, protection, and speedy access for repairs in case of defect in the wires; while the safety and efficiency, as well as economy in the working of the railway, were supplied by the telegraph by signalling every train from station to station and telegraphing generally throughout the line concerning engines and rolling-stock.

In a book issued by him in 1842, entitled, “Telegraphic Railways,” I was startled to find the following:—“To illustrate the practical working of these arrangements under extraordinary circumstances, I will now follow an *express*, and therefore *unexpected*, train in its course from Derby to Leicester.” He then proceeds to describe the process of signalling it through its course. I have examined a “Bradshaw’s Railway Companion” of the time and do not find any express, but only mail and ordinary trains. I think that an *extra* train was meant, for I find the expression used in the examination of Mr. Saunders, the Secretary of the Great Western Railway, on the 6th February, 1840, before a Parliamentary Committee† (among the members of which were Sir Robert Peel, Sir James Graham, Lord Stanley, and Mr. Labouchere, afterwards Lord Taunton). Mr. Saunders stated, in reply to Lord Granville Somerset, that the danger of collision in sending out an *extra* train, without a great interval of time being allowed between it and the ordinary trains, might be guarded against by the use of the telegraph. He also said, in reply to a previous question, that “it perfectly performs all the duty that was expected of it;” and, in reply to another question, “that it would simplify the working and diminish the stock of every description, whether of engines or carriages, besides ensuring greater punctuality; and, in cases of accident, to repair the injury with the least delay.”

By the latter part of 1845 the double and single needle

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\* “Telegraph Manipulation.” By C. V. Walker.

† Fifth Report of the Parliamentary Committee on Railways, and Mech. Mag., vol. xxxiii., 1840, p. 168.

instruments were used everywhere, and the extent and ramifications of the telegraph had so enlarged that the time was ripe for connecting together the whole system, and forming a company for the general transmission of messages and news for the public throughout England and part of Scotland.

In this part of his original scheme, Mr. Cooke was fortunate enough to obtain the co-operation of Mr. John Lewis Ricardo, M.P. for Stoke-upon-Trent and Chairman of the North Staffordshire Railway, through the introduction of Mr. Bidder, who was then the engineer of the London and Blackwall Railway and other lines.

Mr. Ricardo was a man of extraordinary sagacity and great energy. He became, and continued for many years to be, the chairman of the new corporation, which was styled the "Electric Telegraph Company." An Act of Parliament for incorporating the company was applied for in the Session of 1846.

By this time, however, there were other competitors in the field. Mr. Edward Davy had taken out a patent in July, 1838, for a telegraph in which three wires were used, and metallic points attached to magnetic needles were caused to press upon, and so to make various groups of marks upon, chemically prepared calico at the receiving end, the solution employed being hydriodate of potash and chloride of lime. The patent was bought by the Electric Telegraph Company, but never came into use.

Other patents were also taken out in 1841, 1843, and 1845, for a type-printing, an indicating, and an electro-chemical copying telegraph, by Mr. Alexander Bain, of Edinburgh, a most fertile and ingenious inventor, who had previously devised an electric clock.

Another type-printing machine was also patented at the end of 1845, as a communication from Mr. Royal E. House, of the United States, by Mr. Jacob Brett, of which his brother wrote, in 1858, that this instrument "incurred a sacrifice on my part of many thousand pounds, without any valuable result for general purposes."<sup>o</sup>

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<sup>o</sup> "Origin and Progress of Oceanic Telegraphs." By J. W. Brett.

The Electric Telegraph Company found, on going to Parliament, that they were opposed by Mr. Bain, who declared, in his petition, that he had invented an electric printing telegraph, and had previously communicated his invention to Professor Wheatstone. When it came to the Lords' Committee, the Duke of Beaufort, its chairman, told the company's counsel that they had better arrange with Mr. Bain, hinting rather plainly that their Bill might otherwise be thrown out. A compromise was accordingly arranged; the company got their Bill and Bain got £12,000. His patents were transferred to the company, and he entered into an obligation to give the company the use of any further inventions. He was subsequently elected a director of the company.

Bain's prolific genius was soon at work again, and in December, 1846, he patented his electro-chemical telegraph, which consisted of a train of clockwork at each end of the line; at the sending station a paper ribbon, about half-an-inch wide, perforated with holes representing the different letters of the alphabet, as required for the message to be sent, was drawn over a conducting cylinder in connection with the earth and under a metallic spring or style connected to a battery, the other pole of which was connected to the line wire.

At the receiving station a paper ribbon, moistened with an acidulated solution of ferro-prussiate of potash, was in like manner drawn by the clockwork over a metallic cylinder connected to the earth and under a metallic style connected to the line wire.

The clockwork being set in motion, which was done by a current causing an electro-magnet to act upon a detent, the current passed through the circuit, making a blue mark at the receiving end whenever the sending style passed over a perforation and came in contact with the cylinder, and leaving a corresponding blank when the sending style passed over the non-conducting paper between the perforations. The instruments worked with wonderful rapidity; the blue marks appearing to stream out from the recording style as if by magic, so that a number of operators were employed to each instrument to make the perforations in the sending paper, which was done by mechanism causing it to pass

between rollers and under a punch—one hole formed a “dot,” and three a “dash,” of the Morse alphabet.

An experiment was tried in Paris with this electro-chemical telegraph before M. Leverrier and Dr. Lardner, in which a message of 282 words was transmitted through a continuous wire 1,082 miles in length, consisting of two telegraph wires joined together at Lille, making 336 miles, and 746 miles of insulated silk-covered wires in coils.

“A pen,” says Dr. Lardner, “attached to the other end, immediately began to write the message on paper moved under it by a simple mechanism, and the entire message was written in full in the presence of the Committee (each word being spelled completely and without abridgment) in fifty-two seconds, being at the average rate of five words and four-tenths per second. By this instrument, therefore, it is practicable to transmit intelligence to a distance of upwards of 1,000 miles, at the rate of 19,500 words per hour.”\*

I myself often saw, in the year 1847, Bain's telegraph working at an astonishing speed between Manchester and London, and have never been able to understand the cause of its being abandoned.

The only inconvenience was the occasional breakage of the damp paper when handled by the operator; but this was obviated later on by Bain having a disc of prepared paper, like a large filter paper, placed on a metallic plate of the same size, which revolved round its centre, the style having a slow motion from the centre to the edge of the disc, so that it described a spiral commencing at the centre and terminating at the edge.† In this way there was no occasion for the writer of the message to touch the paper.

It will be at once seen that Bain's telegraph was the father of the beautiful and rapid automatic instrument of Sir C. Wheatstone and Mr. Stroh now used at the Post Office, the latest speed of working which, as I am informed by Mr. Preece, is 435 words per minute, or 115 words faster than Leverrier and Lardner's experiment with Bain's telegraph.

This speed of more than 70 distinct currents passing through

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\* “Museum of Science and Art,” vol. iii., p. 117.

† *North British Review*, xlv., p. 559.

the line wire and the instrument in a single second of time calls to mind Juliet's

" Lightning, which doth cease to be  
Ere one can say it lightens."

Nevertheless, having regard to the amazing number of currents generated in a second by the armature of a dynamo-electric machine running at a high speed, I could not say that the limit has been by any means reached with the automatic telegraph.

Let us now return to the Electric Telegraph Company after they had come to terms with Bain and obtained their Act of Incorporation. Foreseeing the possible and, indeed, the probable contingency of other telegraphs being brought forward, they set to work, under Ricardo's guidance, to convert Cooke's contracts with the railway companies from way-leaves into exclusive agreements for a long term, so as to keep any other telegraph from passing over the line. This sagacious policy was successfully carried out, especially in the case of the leading railways and those comprising the great trunk lines from London to the north and west.

By their Act they had the power to lay pipes and wires under the streets of towns, and by the 1st January, 1848, the company opened offices for receiving and transmitting public messages in London, Birmingham, Manchester, Liverpool, and other important places, which could also communicate with the many smaller places and railway stations previously connected up. The only large towns not in communication with the central station in Lothbury were then Bath, Exeter, Plymouth, Brighton, Chatham, Oxford, and Preston.\*

The charges were, however, much too high. A message from London to

Birmingham or Stafford	...	cost 3 $\frac{1}{2}$ d. per word.
Liverpool, Leeds, or Manchester	„	5 $\frac{1}{2}$ d. „ „
York ... ..	„	5 $\frac{1}{2}$ d. „ „
Edinburgh ... ..	„	7 $\frac{1}{2}$ d. „ „
Glasgow ... ..	„	8 $\frac{1}{2}$ d. „ „
Derby, Norwich, Nottingham, or Yarmouth ... ..	„	4 $\frac{1}{2}$ d. „ „

\* *Mech. Mag.*, vol. xlviii., p. 44.



Even a lawyer, under the influence of such a tariff, became suddenly endowed with a power of writing on any subject in a most laconic style, which in his office he would have conscientiously declared to be positively impossible.

The progress of the Electric Telegraph Company was gradual, but never flagged. In 1850, it had 1,786 miles of line and 7,206 miles of wire; in 1860, 6,541 miles of line and 32,787 miles of wire, with 3,352 instruments; and in round figures, when Government contracted to purchase the telegraphs in 1868,

1,300 telegraph stations in Great Britain and Ireland.

10,000 miles of line.

50,000 „ „ telegraphic wire.

8,000 sets of instruments.

3,000 skilled persons in its employ.

3 Continental cables under its control.\*

The year 1850 was notable in the history of the company, because, owing to its high tariffs, a clamour arose for competition, and in that year Acts of Parliament were granted to the Magnetic Telegraph Company and to the British Electric Telegraph Company, which afterwards amalgamated under the name of the "British and Irish Magnetic Telegraph Company," generally known as the "Magnetic Company." I was engineer to the latter company; Mr. Edwin Clark at the time, and afterwards Mr. Latimer Clark, and following him Mr. Culley, filling the office of engineer to the Electric Company. My brother, Mr. Edward Bright, was manager, and in later years also engineer, of the Magnetic Company from its commencement to the Government purchase in 1870. Such of the railways in Great Britain as had not been exclusively secured by the Electric Company were eagerly arranged for by the new company, and nearly all in Ireland, which had not been then thought worth attention by the Electric Company. In this way, competing lines were established on the Lancashire and Yorkshire, East Lancashire, Leeds Northern, Newcastle and Carlisle, Glasgow and South Western, and throughout Ireland. To connect up with London, the Magnetic Company

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\* "Government and the Telegraphs." Effingham Wilson. 1868.

laid a line of ten wires in troughs along the high road by Birmingham to Manchester, continuing six wires to Preston, Carlisle, Dumfries, and Glasgow, with a fork from Dumfries to Portpatrick, for reasons which I shall soon give. On the other side of the Irish Channel another underground line was carried from Donaghadee to Belfast and Dublin, and an isolated branch from Cork to Queenstown.

These underground wires were of No. 16 gauge copper, insulated by a continuous coating of gutta percha.

This remarkable substance, which becomes soft and plastic when placed in hot water, and capable of being moulded, hardening again on becoming cool, was first introduced to this country by Dr. Montgomerie, of the Indian Medical Service, who, observing that the Malays used it for making basins, jugs, shoes, and knife handles, inferred, from the crude native manufacture, that extensive uses would be found for it in Europe. He therefore purchased a quantity, and sent it, in 1843, to the Society of Arts in London.

Its value was speedily recognised; many patents were taken out for its manufacture, and its applications soon became too numerous to catalogue. What concerns us is its qualities of being a good insulator of electricity, insoluble under water, capable of being laid over a wire by being passed through a die when hot, yet being pliable and to a certain extent hard when cold.

Dr. Werner Siemens was probably the first (*viz.*, in 1847) to use it for covering wires laid underground, but a further use of world-wide importance was soon to be found for it.

Gutta-percha covered wires came into use in the tunnel wires on railways in 1847, and soon after for the wires laid in iron pipes under the streets of towns.

In January, 1849, Mr. C. V. Walker laid a length of two miles of No. 16 gauge copper wire, coated with gutta percha, in the English Channel, from the sands near the Pavilion Hotel at Folkestone, where the end was connected to the 83 miles of telegraph erected along the South Eastern Railway to London. The experiment was quite successful, messages being interchanged between the chairman of the company in London and Mr.



Walker, on board the steamer "Princess Clementine," at sea, with the end of the gutta-percha covered wire on board.\*

A plan had been previously matured by Professor Wheatstone, as long before as 1840, for laying a submarine wire, covered with rope, between Dover and Calais, of which full particulars were given by Mr. Sabine in our proceedings; but nothing came of it, and it was not until 1850 that the first telegraph message was sent across the Channel.†

The late Mr. John Watkins Brett, to whom I have referred before in connection with House's Type-printing Telegraph, has justly been generally credited with the merit of having first taken this enterprise seriously in hand.

So early as 1845, he was imbued with the conviction of a submarine telegraph being a possibility, and with his brother, Mr. Jacob Brett, registered, on the 16th June in that year, a company for uniting Europe and America by submarine communication. He did not know of Professor Wheatstone's Channel project, concerning which he afterwards said: "Had these facts then been known to me, I cannot say how far they might have damped my determination to devote my whole time and means to establish and promote the submarine telegraph, and, if possible, to bring this country into instantaneous communication with India and America, then the sole object of my thoughts."‡

An "Oceanic line" was also included in his brother's patent for the printing instrument,§ in which the wires were to be "coated with various colours to distinguish them."

After obtaining permission from King Louis Philippe,|| in 1847, to unite England with France by a submarine cable—followed by a concession from Louis Napoleon, when President of the French Republic in 1849—a single copper wire, covered with gutta percha to the diameter of half-an-inch, was laid on the 28th August, 1850, from Dover to Cape Grisnez, the greatest depth passed over

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\* "Electric Telegraph Manipulation, p. 102." Walker.

† *Journal of the Society of Telegraph Engineers*, vol. v., p. 90.

‡ *Proceedings Royal Institution*, March 20, 1857.

§ No. 11,010 of 1845.

|| "Origin and Progress of the Electric Telegraph, p. 63." J. W. Brett.

being thirty fathoms. At every sixteenth part of a mile a leaden clamp or weight was securely fastened on to secure it in position. Messages were interchanged between the steamer "Goliath" and the English shore while the wire (which was coiled on a large drum) was being laid, and afterwards between the two shores. Next morning, however, the circuit was broken, and it was found that the action of the waves had rubbed the wire upon the rocks, and destroyed the coating of gutta percha.

The French concession stipulated that "unless the experiment shall result in a favourable execution by the 1st September, 1850, the right conceded shall revert to the French Government." This object was attained, but nothing more.

A copy of the messages transmitted was attested by some ten persons, including an engineer of the French Government, who was present to watch the proceedings; this was forwarded to Paris, and a prolongation of the privilege was granted.

A more substantial cable was manufactured next year; the conducting wires consisted of four No. 16 gauge copper wires, surrounding a heart of tarred hemp, and covered with a bedding of the same material. The conductors were double-covered with gutta percha to the diameter of a quarter of an inch, the core being made at the Gutta Percha Company's Works, then under the management of Mr. Samuel Statham, a gentleman of great energy and business capacity.

The outer covering was made of galvanised iron wires, laid on the core like an iron-wire pit rope. I am unable to say by whom this mode of sheathing was originally suggested, but Mr. Cramp-ton, the engineer of the company (with whom was associated Mr. Wollaston), gave the order\* for its application to Messrs. Wilkins & Weatherly, of High Street, Wapping.

They were, however, stopped by an injunction of the Court of Chancery, for infringing the patents of Mr. R. S. Newall, of 1840 and 1843,† and the manufacture was carried on by his firm. The cable, 24 miles in length and 180 tons in weight, was delivered

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\* See letter in *The Times*, Nov. 12, 1852, from Messrs. R. S. Newall & Co. of Gateshead-on-Tyne.

† Nos. 8,594 of 1840, and 9,655 of 1843.

on board the "Blazer," a hulk provided by our Government. Some delay arose from the litigation, and the manufacture was not finished until the 17th September. Mr. Crampton, who had contributed a considerable part of the capital for the undertaking, now took the laying in hand. During two stoppages, one from cessation of signals from the shore, the other from the parting of a tow-rope, a good deal of cable was wasted, and the length of cable finally proved too short by a mile. This, however, was made soon after, and spliced on to the laid part of the cable.

This cable is still in working order, although many new lengths have been inserted from time to time to make good defects caused by damage from ships' anchors and deterioration. Its type, so far as the sheathing is concerned, continues to be the model for all submarine cables laid in comparatively shallow water. A piece of the cable, as actually laid, was exhibited by Mr. Brett at the Exhibition of 1851, in Class X.; also a vertebrated iron tubular cable, and another constructed with the addition of a chain of links for the purpose of giving greater strength in dangerous situations. Drawings of these are given in the Report, and a Council Medal was awarded for them. The Submarine Telegraph Company (formed by Mr. Brett, Sir James Carmichael, Lord de Mauley, and others) soon contracted with Messrs. Newall to lay a cable to Ostend. This company was the first to value in this country, and to adopt, the marvellous printing instrument of Professor Hughes (our late President), so well known by his other inventions—as, for example, the microphone. His printing telegraph has been greatly employed by many foreign Governments, and afterwards again by our United Kingdom Telegraph Company here; and, as we all know, he was the Royal Medallist of the Royal Society last year. The instrument of which I speak is unequalled as a wonderful example of the greatest mechanical skill in its synchronism and all its working parts, and also of ingenious electrical application thereto. Other cables were then laid by the Magnetic Company to Ireland—between Portpatrick and Donaghadee—and the Electric Company to Holland, with others, into the details of which time does

not allow me to enter.\* These cables were, with differences in the number of wires and dimensions, all, with one exception—that between Varna and Balaclava—made in the same style as regards the outer sheathing, namely, with outer iron wires, the wire not being *twisted*, but laid on spirally, with what is termed a sun and planet motion, in which each bobbin of wire makes one turn in the opposite direction for every revolution of the machine. At the end of the year 1855, the North American lines were laid as far as Newfoundland, and in Europe the Magnetic Company's lines were completed as far as the west coast of Ireland.

The practicability of uniting the great telegraphic systems, by means of a submarine cable between the shores of the Old and New Worlds, had for a long time engaged the thoughts of some of the most enterprising men of science and of experience in telegraphs. It was yet to be seen whether a cable could be laid in such great depths of water, and continuously for so great a distance, without mishap. But there was also another problem unsettled: Could so long a circuit be worked electrically? When wires are fixed to insulators upon poles in the usual familiar manner, there is no difficulty in telegraphing through the longest circuits which can conveniently be used; but when the wires are coated with gutta percha, inductive action comes into play, and a considerable retardation of the signals takes place, arising from the wires acting in the same manner as a Leyden jar.†

Having a great length of underground gutta-percha covered wire under my control as engineer of the Magnetic Company, I carried out a long series of experiments by having the wires connected up backwards and forwards between London and Man-

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\* Particulars, with drawings and sections of the earlier cables, are given in "The Electric Telegraph," by Dr. Lardner. New edition, revised and rewritten by Edward B. Bright, F.R.A.S. James Walton, 187, Gower Street. 1867.

† There are several papers on this subject in the *Reports of the British Association* in 1854: "Experimental Observations on an Electric Cable," by Wildman Whitehouse; "On Magneto-Electricity and Underground Wires," by Edward B. Bright; "On Improvements in Submarine and Subterraneous Telegraph Communication," by C. F. Varley.

chester, so as to form a continuous circuit of a length equal to that of a telegraph cable between Ireland and Newfoundland, or more than 2,000 miles. My method was to use a succession of opposite currents, which I had previously found to be successful with the magneto-electric instruments used by that company. I could only try my experiments at night, or on Sundays, when the traffic on the line was small.

Mr. Whitehouse, a gentleman of very high intellectual power, and a most ingenious and painstaking experimenter, had been working in the same direction for some time upon the wires of some Mediterranean cables connected backwards and forwards, so as to get a length of about 900 miles.\* The use of these wires had been allowed him by Mr. Brett, who was founder of the Mediterranean Electric Telegraph Company, and on my talking of my experiments with the latter one day at Greenwich, in 1855, he brought Mr. Whitehouse and myself together, the result being that we continued our researches thereafter conjointly until the beginning of the Atlantic line, when we had to divide our labours; he becoming the electrician, and I the engineer of the company.

In July, 1856, Mr. Cyrus Field, the deputy-chairman of the New York and Newfoundland Telegraph Company, left America for London empowered by his associates to deal with the exclusive concession possessed by that company for the coast of Newfoundland, and other rights in Nova Scotia. He had, for some time, been concerned with Mr. Brett in the project. He had been here before about telegraph business, and I had discussed the Atlantic line with him in the previous year. On the 29th of September, 1856, an agreement was entered into between Mr. Brett, Mr. Cyrus Field, Mr. Whitehouse, and myself, by which we mutually engaged to exert ourselves *"with the view and for the purpose of forming a company for establishing and working electric telegraphic communication between Newfoundland and Ireland, such company to be called the 'Atlantic Telegraph Company,' or by such other name as the parties hereto shall jointly agree upon."*

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\* See *Illustrated London News*, October 6, 1855, for drawing and description of Mr. Whitehouse's apparatus. Also *Engineer*, January 30, 1857.



Mr. Field was—I am happy to say *is*—a man of extraordinary energy and power; rapid in thinking and acting, and endowed with courage and perseverance under difficulties—qualities which are rarely met with.

Professor Morse, the electrician of the Newfoundland Company, had also arrived in London, and Mr. Whitehouse and I showed him one night—the 9th October, 1856—at the office of the Magnetic Company in Old Broad Street, that signals could be sent at the rate of 210, 241, and, in one experiment, at the rate of 270 signals per minute, through the continuous circuit of 2,000 miles of the company's underground wires between London and Manchester.

The wires were joined backwards and forwards at Manchester and London; in each loop at both ends a galvanometer being inserted in the circuit to prove that the currents really passed through. By this the resistance, though not the *retardation* of the line, was largely increased.

On the 20th October, 1856, the Atlantic Telegraph Company was registered, Mr. Brett heading the subscription list with £25,000, Mr. Field following him for the same amount; we then held meetings in Liverpool, Manchester, and Glasgow, which were addressed by all of us the Founders, and nearly the whole of the capital, consisting of 350 shares of £1,000 each, was subscribed for in a few days, principally by shareholders in the Magnetic Company.

I have no time to give a detailed account of the Atlantic Telegraph; it would alone occupy an entire evening, and there are several books in which it is described.\*

I cannot, however, leave this hiatus without telling you what pleasurable recollections I have of those who assisted me so ably in my duties (as engineer-in-chief) in the last voyage. Mr. (now Sir Samuel) Canning, who had laid several cables; Mr. Henry Woodhouse, also an old cable layer; Mr. Everett, of the U.S.N.; and Mr. Clifford, now engineer to the Telegraph Construction Company. Irrespective of our business relations, we were all

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\* e.g.—“The Atlantic Telegraph,” by W. H. Russell, LL.D. Illustrated by Robert Dudley. Day & Sons. “The Electric Telegraph,” by E. B. Bright. James Walton, Gower Street. 1867.

colleagues and friends together, and I never had a happier time on board ship.

Mr. Whitehouse was equally fortunate in regard to Mr. J. C. Laws, Mr. Saunders, Mr. de Sauty, Mr. Collett, and other members of his staff, whose names I cannot now call to mind.

I finally landed the first Atlantic cable at Valentia on the 5th August, 1858; and it is worthy of remark that just 111 years previously, on the 5th August, 1747, Dr. Watson astonished the scientific world by practically proving that the electric current could be transmitted through a wire hardly two miles and a half long—nevertheless he showed at the same time that the earth could be used for the return circuit.

The first messages which passed through the cable were one from the Queen to the President of the United States, and his reply. Many others followed of some importance; but this cable broke down on the 3rd September. Various causes have been suggested for this: too high electric power being used, an unusually violent lightning storm at Newfoundland, and a supposed factory fault masked by the tar in the hemp, &c. I cannot give any opinion (as the cable was reported by the electricians in excellent condition after being completed by me, when it passed out of my charge), except that I agreed with Mr. Brett that the manufacture had been pressed forward with too much haste;\* which view was shared in by Mr. Canning† and Mr. Woodhouse.‡ Mr. Whitehouse had wished to test every separate coating of gutta percha during manufacture, but there was not sufficient time allowed for that to be done.

The next great submarine telegraph cable was laid from Suez, in several sections between Suez and Kurrachee, at the end of 1859 and in 1860. 3,499 miles were made and laid by Messrs. Newall & Co. for the Red Sea and India Telegraph Company, under a guarantee from Government. It was a most disappointing failure, the sections breaking down one after the other; indeed, they do not appear to have been all at work at any time for the

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\* "Evidence before Submarine Telegraph Committee," Q. 1,443.

† " " " " Q. 1,498.

‡ " " " " Q. 991.

30 days of continuous through working stipulated in the contract.\*

In May, 1859, the Government, through Sir Stafford Northcote, then President of the Board of Trade, asked the advice of Mr. Robert Stephenson and myself respecting the form of cable to be used for a submarine line which it was proposed to lay from Falmouth to Gibraltar, 1,100 nautical miles in length, and from 100 to 2,500 fathoms in depth. In my report I recommended a much larger copper conductor than ever used before, to weigh  $3\frac{1}{2}$  cwt. per nautical mile, or 392 lbs., and the same weight of gutta percha as the insulator.

This was precisely the same core which I had recommended the Atlantic Company to adopt in 1856, but it had all been settled by contract before I became their engineer. The relative figures in the Atlantic Cable were 107 lbs. of copper and 261 lbs. of gutta percha; those of the Red Sea 180 lbs. of copper and 212 of gutta percha per knot.†

For the outer covering of the proposed Gibraltar cable I advised different forms for the various depths—of great strength and low specific gravity for the deepest water, and of greater specific gravity for the smaller depths. This part of the report is not, however, of any importance, as the Government changed their plans, and in March, 1860, decided on laying a cable between Rangoon and Singapore; and the lighter cable, which was intended for laying in the deep water in the Bay of Biscay, and which would have been quite unsuitable for the Malay coast, was abandoned. Again, in January, 1861, in consequence of the war with China having come to an end, it was settled that it should be laid from Malta to Alexandria, and divided into sections at Benghazi and Tripoli. It was accordingly laid in the summer of 1861, with great success, by Messrs. Glass & Elliot, to whom the contract for laying was given by the Government. Mr. H. C. Forde was the engineer, and

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\* The Submarine Cable Committee's Report furnishes all the evidence about this unlucky cable. Tight laying, overheating, excessive battery power, and fissures in the gutta percha, are all spoken of.

† "Appendix to Report of Submarine Telegraph Committee," p. 482.



Mr. (afterwards Sir) C. W. Siemens the electrician for the Government. Full details of the construction, laying, and testing of this cable were given in two papers read at the Institution of Civil Engineers.\* The work throughout appears to have been carried out with great care, and due regard to the conditions taught by experience to be necessary in submarine telegraphy.

The Report and Evidence of the Submarine Telegraph Committee was published during the construction of this cable. I consider it to be the most valuable collection of facts, warnings, and evidence which has ever been compiled concerning submarine cables, and that no telegraph-engineer or electrician should be without it if beyond reach of access to it. It is like the boards on ice marked "Dangerous" as a caution to skaters.

The next great submarine cable enterprise was the Persian Gulf line, uniting the Turkish land telegraphs at the head of the Gulf and the Persian land telegraphs at Bushire with Kurrachee; the failure of the Red Sea line, and the loss of £800,000 over it, having led the Government of India to adopt the alternative telegraphic route by way of the Tigris Valley. A careful line of soundings was completed by Lieut. Stiffe (of what was then called the Bombay Marine) in 1862; and, the bottom being favourable for a cable, the Government of India resolved to lay one, of great strength and durability, designed by Messrs. Bright and Clark, and appointed us, in the autumn of 1862, engineers to the work. The late Colonel Patrick Stewart, R.E., was the very able Government Director of the entire line.

The total length of cable was 1,450 miles, weighing no less than 5,028 tons, being by far the heaviest length previously dispatched on one submarine expedition. The copper conducting wire

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\* *Proceedings of Institute of Civil Engineers*, vol. xxi. The first use of resistance coils for testing cables was with this cable; they had been used before for testing underground wires. See Specification of Patent, E. B. & C. T. Bright No. 14,331, of 1852. The part referring to this mode of testing with known resistances is published in the "Submarine Telegraph Committee's Evidence," p. 53, with drawings; also, the "Museum of Science and Art," 1854, and "The Electric Telegraph," by Lardner & Bright, p. 76, edit. 1867.

weighed 225 lbs. to the knot, and the conductivity of the copper was raised to a higher point than had been attained before, amounting to nearly 90 per cent. of pure copper. In many of the older submarine cables laid before this point had received attention, the conductivity was as low as 40 per cent., and even lower.

The copper wire was made of a segmental form, surrounded by a tube, all being drawn down from a large built-up copper rod of the same construction; so that, while the wire was smooth and no interstice could be seen, it was the same mechanically as a strand, but superior electrically. The weight of gutta percha was 275 lbs. per knot. The Government of India contracted with the Gutta Percha Company for the core.

The laying on of the outer sheathing was put up by tender upon our specification, and the contract was obtained by Mr. Henley, of North Woolwich. In putting on the outer sheathing, a wet serving of hemp was first applied with the view of discovering any defect in the insulator at once; the outer covering of 12 No. 6 B.W.G. galvanised iron wires was then applied, and over it a protective coating, respecting which I must say a few words, as it was afterwards applied to so many and such great lengths of telegraph cable. A patent was taken out in 1858 (No. 1,965) by Messrs. Clark, Braithwaite, and Preece, for applying a covering of asphalt and hemp to the outer wires of a finished iron cable, with the object of retarding the decay of the zinc coating the iron wires of cables. Mr. Clark had acquired the interest of Mr. Preece (not our esteemed Past-President), and, as Mr. Clark and I were in partnership at the time, I purchased Mr. Braithwaite's interest in it in 1860. It had been tried on a short length of cable laid to the Isle of Man from near Whitehaven, in 1859. The cable was passed through the hot asphalt contained in a revolving tank enclosing the bobbins of hemp. The mixture was made hot by charcoal fires outside. The insulation was damaged by this process, and the delay and injury was so great that it took more than a fortnight to get the 36 miles covered with it. Mr. Clark went away to the Red Sea in 1861. Nobody would use the patent after the Isle of Man cable experience, so I set to work to deal with the matter.

The result of my study and experiments was that I devised and patented (No. 466, A.D. 1862) the system generally adopted in all cable works since, of applying the hot compound over the finished cable by an elevator driven from the machine, making the laying on of the hemp or jute and compound one operation, and saving the delay of the former double manufacture, and also that, as the supply of compound was arrested (if the closing machine was stopped for putting in a bobbin of fresh iron wire), no damage could be caused to the insulator. The cable was then passed through semi-circular rollers (a stream of water being poured over them), by which the coating was thoroughly pressed into all the interstices of the wires. Thus the coating was done at the same time by part of the cable machinery, and the danger of destroying the insulation, and the cost, delay, and damage of re-coiling was avoided. Moreover, after a great series of experiments, I arrived at an improved composition of mineral pitch, tar, and silica made from calcined flints ground to a powder, by which latter addition the boring tool of the teredo was damaged directly it touched it. This process has been highly successful, practically as well as pecuniarily, having yielded upwards of £30,000 to Mr. Clark and myself up to the time that my patent expired.

I shall say no more about the Persian Gulf Cable. I went out to lay it myself, and everything went on without any hitch or mishap. All the details will be found (including a new mode at that time of joint testing) in a paper read by me at the Institution of Civil Engineers in 1865. The cable was, and has been, a complete success.

By the date of the laying of the Persian Gulf Cable, forming the first telegraphic connection between England, Europe, and India, the science of making and laying submarine lines was pretty definitely worked out, and no important improvement has been since introduced. I do not therefore propose to add further particulars relating to subsequent cables, the more so as they are to be found in many books, including our own journals and technical papers.

I should, however, observe that during my absence in India

and Persia, in 1864, combinations were arranged leading to the formation of large cable companies, which materially and beneficially affected the future telegraphic communication across the seas.

In March of that year, the India Rubber, Gutta Percha, and Works Telegraph Company was registered to take over the large works of Messrs. Silver—now generally called the “Silvertown Company”—and in the following month the businesses of the Gutta Percha Company and Messrs. Glass, Elliot, & Company were combined as the “Telegraph Construction and Maintenance Company.”

At the present time these companies and Messrs. Siemens have constructed very great lengths of cable, making the total 107,000 miles of submarine communication with all the important parts of the world, in which more than £37,000,000 capital is invested, according to calculations supplied me by Sir James Anderson.

I will advert to one further point before quitting the subject of submarine cables:—The importance of thoroughly surveying and sounding the bottom along the route where a cable is proposed to be laid has been of late years more closely carried out than in early times, and I may remark that had this been always done many of the failures of early cables after but a brief submergence would not have taken place.

It is only by means of taking soundings, which should not be more than ten miles apart, that an even bed can be relied upon.

Whilst laying the Lisbon-Madeira Cable in 1874, the Telegraph Construction Company's ship “Seine” discovered a bank in latitude  $33^{\circ} 47' N.$  and longitude  $14^{\circ} 1' W.$ , the depth being about 100 fathoms, with 2,400 fathoms in its immediate neighbourhood.

In 1879, Messrs. Siemens' telegraph ship “Faraday” discovered shallow water in mid-Atlantic when laying the Atlantic Cable of that year.

These soundings, being of extreme practical importance for the welfare of submarine cables, are also of advantage to the scientific world generally, in increasing the knowledge of the depths of the sea and the nature of its bed.

The Silvertown Company has given particular attention to this subject, as described in a paper, "On Oceanic Shoals discovered in the Steamship 'Dacia,'" read before the Royal Society of Edinburgh, in 1885, by Mr. J. Y. Buchanan, F.R.S.E.

Before starting to lay the cable, the telegraph steamers "Dacia" and "International," in 1883, sounded from Cadiz towards the Canaries on two separate zig-zag routes; and over 100 soundings were taken in this way by each ship at distances of 10 miles apart.

In the course of these soundings the "Dacia" discovered a bank in latitude  $31^{\circ} 9' 30''$  N. and longitude  $13^{\circ} 34' 30''$  W., and depth 58 fathoms, in the vicinity of a 2,000-fathom depth; or, about the height of St. Paul's Cathedral from the bottom to the surface as compared with some of the highest Swiss mountains. These soundings were in what otherwise might have been taken as the line of the Canaries Cable.

In 1885, and last year, the telegraph steamer "Buccaneer," belonging to the same company, made close surveys and soundings down the West Coast of Africa to a point as far south as St. Paul de Loanda, with a view to laying cables there; and the results were given by Mr. Buchanan in a paper read at the Royal Geographical Society in November last. There were over 1,000 soundings taken, besides noting the strength of the currents along the coast, which is considerable.

I regret that time does not now allow me to do more than refer briefly to the means of doubling the transmitting power of cables by the highly ingenious duplex system invented by the Messrs. Muirhead, which is now applied most successfully to about 50,000 miles of submarine lines.

Duplex telegraphy as applied to cables has been brought to a high degree of perfection, judging from the results that have been obtained on the Mackay-Bennett cables recently laid across the Atlantic. The system is simply a "bridge" method, in which two sets of condensers are kept balanced in connection with the cable and the artificial cable, without the insertion of wire resistance to any great extent.

Dr. Muirhead informs me that on the New York and Canso cable of 826 knots the ordinary working rate was 21 words per

minute, but when the duplex system was applied the rate of transmission was actually doubled, being 42 words per minute. Also, that on the long Atlantic cable of the same system between Canso, Nova Scotia, and Waterville, Ireland, of 2,353 knots, while the ordinary telegraph instruments gave a speed of nearly 16 words per minute, the duplex apparatus has yielded  $30\frac{1}{2}$  words—equal to an increase of  $93\frac{1}{2}$  per cent.

Referring again to the English telegraph companies and to their acquisition by the State; opinions have often been expressed that the Governments of the time made an improvident bargain, but I think I shall be able to show that such was not really the case.

While the telegraph lines of the various countries in Europe, Asia, India, and in our Australian colonies, were erected and worked by their respective Governments, in England not only the first telegraphs were started by private enterprise, but so carried on for 33 years (1837 to 1870) until purchased by Government.

During this long period those engaged in the undertaking had provided the capital, incurred all the risk, and developed the telegraph system into a highly lucrative business, from which the profits, notwithstanding competition, were steadily increasing—so much so, that the net earnings of the two largest companies (the Electric and Magnetic) ranged from 14 to 18 per cent. per annum.

The companies were not desirous of parting with the systems they had created, but the transfer to Government was very strongly advocated by the press and others.

Without, however, giving the companies particulars beforehand, a Government Bill was brought forward suddenly, as shown by the following extract from a pamphlet published at the time by Effingham Wilson, entitled "Government and the Telegraphs":—

"On Wednesday, the 1st April, 1868, the new Chancellor of the Exchequer, Mr. Ward Hunt, appeared at the table of the House of Commons to move for leave to introduce one of those anomalous measures known in Parliamentary phraseology as 'hybrid' bills (*i.e.*, public bills affecting private rights), to enable Her Majesty's Postmaster-General to acquire, work, and



maintain Electric Telegraphs. . . . Mr. Ward Hunt rose to ask leave to introduce this Bill at 25 minutes before 6 o'clock. The House of Commons adjourns its Wednesday discussions at a quarter before 6 o'clock. The Chancellor of the Exchequer had, therefore, only ten minutes to develop 'the objects' of the bill. Having fully exhausted those ten minutes, the Speaker intimated that the hour for terminating the discussion had arrived.

"Mr. Milner Gibson and Sir Charles Bright rose to address the House, but they were too late even to ask a question or obtain an answer, much less to raise any discussion on the principle of the measure."

The bill as at first framed was very arbitrary, and practically looked like confiscation; but, in view of the strong opposition of the companies, the Post Office authorities came to terms by agreeing to 20 years' purchase of the net profits—that is to say, that Government acquired property yielding them 5 per cent. on the amount they paid, which they were able to raise at about 3 per cent.

A Parliamentary committee, consisting of the Chancellor of the Exchequer, Mr. Goschen, and others, then sat nine days, and thoroughly thrashed out the conditions of the bill in July, 1868, while a Conservative Government was in power, and the terms arrived at were confirmed next year by the Money Bill brought in by a Liberal Government.

Before the completion of the purchase at the beginning of 1870, the accounts of the companies were thoroughly investigated by accountants of the Post Office, and the existing plant was examined by their experts.

The total paid to the Telegraph Companies was £5,847,347, of which the Electric and Magnetic had £4,182,362, the balance being divided between the following smaller companies: United Kingdom, Reuter's, and London and Provincial.

Most of the railways had telegraphs of their own, and derived revenue from messages, and also in some cases from payments made to them by the telegraph companies for way-leaves and other privileges. The value of these to Government was subsequently fixed by arbitration at £1,817,181.

Upon national considerations the Government quickly extended the wires to a vast number of small places, which, though a great boon to the community at large, entailed a very large extra cost, especially as the capital outlaid in such extensions has been debited in the accounts as part of the charges against the revenue of each year. The reduction of the message rates to one shilling also entailed loss at first. These facts account for the Postal Telegraph Department not having profits to show such as made by the companies.

Bearing upon this, I may mention that the companies handed over, in 1870, 48,378 miles of land wire and 1,622 miles of cable wires (irrespective of the railway wires worked by them), connecting together 2,488 telegraph stations; while at the present time the Post Office have no less than 153,153 miles of wire (including submarine wires) used for commercial purposes in communication with 5,097 offices. Thus the mileage has been trebled and the stations doubled. There are also 17,042 miles of wire used for private purposes. In addition to this the railway companies have about 70,000 miles of wire. Making a gross total of 240,195 miles; and the weight of the iron wires employed is no less than 50,150 tons.

The following comparison of tariffs and messages for the past 30 years is interesting:—

Year.	No. of Messages.	Tariff and Remarks.
1855 ...	882,360 ...	1s. 6d. to 4s. Address free.
1860 ...	1,863,839 ...	Do. do.
1865 ...	4,650,231 ...	{ 1s. to 2s., and a 6d. rate in certain large towns.
1869 ...	*7,500,000 ...	{ * Estimated for year preceding transfer.
1870 ...	9,850,177 ...	{ 1s. tariff for 20 words. Address free.
1875 ...	19,253,120 ...	
1880 ...	26,547,137 ...	
1884-5... †	33,493,224 ...	
1885-6... †	47,508,509 ...	{ 6d. tariff for 12 words. Address counted. Also October to October.

I should, however, remark that on separating the inland



messages in the last two returns from the foreign, press, &c., messages, the great immediate increase by the change of tariff becomes more evident, as these figures give 24,615,395 messages in 1884-5 sent at the shilling, and 37,692,249 in 1885-6 at the sixpenny rate; and for the last six months under review the inland sixpenny messages were at the rate of more than 42 millions per annum. The receipts have also turned the corner, showing an increase of £5,526 over the corresponding period of the shilling rate in 1885. This is partly accounted for by more *extra* words being now used by senders, the average amount paid per message being now 8½d., while under the shilling rate it was 1s. 0½d.

I am indebted for these later statistics to the Postal Telegraph authorities, Mr. Patey, C.B., Mr. Graves, and Mr. Preece, F.R.S.—all members of our Council. If time had permitted I intended to describe Professor Sir William Thomson's beautiful apparatus for working through long cables, and also his system of taking deep-sea soundings by the use of a small steel wire, which is generally adopted. Nor have I been able to refer to telephones, which may be regarded as a branch of the telegraphs.

In conclusion, I beg to thank you for the consideration and patient attention you have accorded to my long address.

MR. E. GRAVES: Gentlemen, I am sure you will all agree with me that it is impossible to part this evening without returning our hearty thanks to Sir Charles Bright for the very lucid particulars that he has delivered to us. In a brief space he has condensed a great number of facts and figures, so that nearly every sentence bristles with them. He has related the development of telegraphy in this country, and of submarine cables provided by British capital and British enterprise, in a way that is the more interesting because in the story detailed there was much personal interest, as he himself took a conspicuous part in many of the achievements related. I beg to move, "That the thanks of the Society are due to the President for the valuable and interesting address just delivered by him, and that he be requested to allow it to be published in the Journal of the Society."

MR. W. T. ANSELL: Permit me to second the proposition. I

am quite sure that you will give Sir Charles Bright your warmest and unanimous thanks for his able review of the early rise and progress of telegraphy. It affords me very great pleasure to second this proposition, because I believe I am almost the oldest worker in the early fields of telegraphy still in active service, and one of my early colleagues was the present Sir Charles Bright, and another colleague was his brother Mr. Edward Bright, while others I am happy to say are still with the minority, and I hope they will remain with us for many a long year. After listening to this very able address, many of our young friends whom I see around me will understand, to some extent, what were the early troubles with which the pioneers of telegraphy had to contend: what they *really* were, none but those who took part in the operations can ever know.

Mr. E. GRAVES: Gentlemen, you have heard the resolution which has been moved and seconded. May I ask you to express your approbation?

The motion was carried unanimously.

The PRESIDENT: Gentlemen, I beg to thank you very sincerely.

A ballot took place, at which the following were elected:—

*Member:*

P. B. Walker.

*Associates:*

W. J. S. Barber-Starkey.

Walter G. A. Bond.

J. Sandford Coathupe.

William Green.

Frederick M. Hodgson.

James Kynoch.

Albert Edward Morrison.

Algeron Richard Nevill.

Walter A. Purdom.

Robert Ritchie.

W. B. Sayers.

Arthur Annesley Voysey.

*Students:*

Charles Arnold Healing.

William Edward Hayne.

William Peto.

Charles Mott.

Alexander Gilbert Sanders.

Herbert Thomas Sully.

The meeting then adjourned.

The One Hundred and Sixty-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 27th, 1887—Sir CHARLES T. BRIGHT, President, in the chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates were announced, and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

E. R. Barker.	1	C. H. Reynell.
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**From the class of Students to that of Associates—**

Peroy Gilbert Ledger.

Donations to the Library were announced as having been received since December 9th, 1886, from Lord Rayleigh, the Newcastle-on-Tyne Public Library, Mr. John Aylmer (Local Honorary Secretary, Paris), and Professor Silvanus P. Thompson, to whom the thanks of the meeting were heartily accorded.

The following paper was then read:—

## TELEPHONIC INVESTIGATIONS.

By Professor SILVANUS P. THOMPSON, D.Sc., Member.

Just a decade ago electricians in both hemispheres were directing their attention to the perfection of instruments for the electrical transmission and reproduction of sounds. On this side of the Atlantic, Mr. Varley and Messrs. C. and L. Wray, and on the other, Messrs. Elisha Gray, Graham Bell, and T. A. Edison, were all at work. The years from 1875 to 1879 witnessed a marvellous series of improvements in telephonic instruments of all kinds, and the commencement of the exchange system.

Since the latter year the development has been almost wholly of a commercial nature. If we except the researches of Graham Bell and Tainter upon the photophone, which have as yet not been made of any commercial value, it is just to say that no great scientific progress was made between 1879 and 1885; for Professor Hughes's discovery of the microphone was made before this in 1878, and the much-applauded methods of Van Rysselberghe for the perfection of long-distance telephony are a mere commercial extension of the researches of Black and Rosebrugh in 1878-9.

Yet it cannot be said that the lull in telephonic invention and discovery has arisen from the absence of need for further improvements. The need exists for better, cheaper, more reliable instruments—for instruments less subject to interference from currents which stray into the system by induction and by leakage—for instruments which will work over longer distances. It would be strange indeed if such obvious and well-known necessities did not sooner or later engender invention. Already, indeed, in several quarters a revival of attention to this subject has been awakened. In our own metropolis Mr. Langdon Davies has produced a series of inventions which might well claim the attention of this Society, comprising as they do that novel kind of transformer the Phonopore, the Phonoporic Telegraph, and the Open-circuit Telephone. Already we hear of duplex and even multiplex telephony—of special methods for long-distance telephoning, and of other kindred advances. A paper recounting the inventions thus looming somewhat dimly before us might form the subject for a profitable discussion: there would at least be no lack of matter. The aim of the present writer is a far simpler and more personal one. For some three or four years he has been engaged in the intervals of other duties in examining various points in the theory and practice of telephony, and his researches have resulted in several forms of instruments, some of which possess new points of scientific interest, whilst others do not profess to be more than useful commercial articles in which the points of novelty are solely of an industrial order. For some months past the writer has been anxious to submit his

work to public scrutiny, and to breathe the healthy atmosphere of criticism; but his ardour has been controlled by friendly advisers who counselled delay, but who, now that all fears have proved needless, have left him the desired freedom of speech.

A complete telephonic system (we are not here dealing with exchange working) usually consists of at least four parts:—

1. A transmitter.
2. A transformer, or induction-coil.
3. A circuit of outgoing and returning wires (or earth).
4. A receiver.

For transmitting both ways, all parts, save No. 3, are usually duplicated. In certain instances the transformer is dispensed with. In certain other instances transformers are used at both ends of the line—one at the transmitting station, the other at the receiving station. In some instances, though not in the first telephones nor now usually, the receiving and transmitting instruments are identical. In fact, we may classify the various systems as follows:—

I. Transmitter  $\begin{matrix} \text{Line Wire} \\ \text{Return Wire} \end{matrix}$  Receiver.

This is the oldest system.

II. Receiver  $\begin{matrix} \text{Line Wire} \\ \text{Return Wire} \end{matrix}$  Receiver.

This is Graham Bell's system; either receiver being used as transmitter.

III Transmitter — Transformer  $\begin{matrix} \text{Line} \\ \text{Return} \end{matrix}$  Receiver.

This is the one in common use.

IV. Transmitter — Transformer  $\begin{matrix} \text{Line} \\ \text{Return} \end{matrix}$  Retransformer—Receiver.

As the writer's investigations relate to transmitters, transformers, and receivers, it may be well to take up the subjects in this order.

#### TRANSMITTERS.

It may be convenient here to give a systematic table of all the principal classes and genera of transmitters. The three classes relate to the way in which the sonorous vibrations are made to

affect the electric system, by varying (1) the resistance; (2) the electro-motive force; (3) the capacity of the circuit or of a part of it.

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### CLASSIFICATION OF TRANSMITTERS.

#### I. *Varying Resistance*—

##### 1. Loose Contact

with diaphragm ... Reis, Berliner, Blake, etc.

without diaphragm... Hughes.

##### 2. Liquid Resistance

in external circuit ... Yeates, Gray, Graham Bell, Edison.

in battery ... Graham Bell.

liquid jet ... Graham Bell, Ch. Bell, and Tainter.

##### 3. Short-circuiting ... Edison.

##### 4. Compressible semi-conductor ... Edison.

##### 5. Photo-electric ... Graham Bell, and Tainter.

##### 6. Tribo-electric ... Edison ("Motograph").

##### 7. Crookes' Layer ...

#### II. *Varying Electromotive Force*—

##### 1. Magneto-electric methods:—

Moving conductor in magnetic field	} (See <i>Receivers</i> )
Varying magnetic field around conductor ...	

Varying coefficient of mutual induction ...	Spottiswoode.
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Varying coefficient of self-induction	G. J. Stoney.
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##### 2. Electro-capillary... A. Breguet.

##### 3. Thermo-electric ...

#### III. *Varying Capacity*—

##### 1. Vibrating condenser plates ...

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My own researches in this part of the subject have been somewhat desultory. So far back as the end of 1878 I observed that if a thermopile is placed so as to receive the radiations

from a singing flame, a sensitive receiver of appropriate low resistance placed in the circuit will repeat the vibrations. I devised at that date a special transmitter, in which the voice vibrations were led into a Koenig's manometric capsule, so that the gas flame connected therewith, being thrown into vibrations in correspondence with the voice, might affect a thermo-electric transmitting apparatus.

Later, I worked at the problem of securing a more direct correspondence between the voice-waves and the electric impulses of the transmitter than was the case with many of the transmitters which were used in great variety in the years 1878-82. The circumstances which in such instruments as the Blake transmitter and in the Wollaston transmitter (commonly known as the Gower-Bell) occur to cause this lack of correspondence are various. All diaphragms have tones of their own—not always well defined, but still obviously present. A diaphragm having a low fundamental tone gives the voice a “boomy” sound, while a diaphragm of too shrill a tone gives the voice a “tinny” sound. Moreover, all springs have tones of their own. Further, instruments such as the Blake transmitter, the operation of which is to open or close the contact to a greater or less degree in proportion to the vibrations, do not always do so. Some sudden, powerful sound taken up by the diaphragm gives the light contact-mechanism behind it such a sudden forcible thrust that the electrodes are forced wholly, instead of partially asunder, and the resistance, instead of varying in any exact inverse proportion to the displacement, increases suddenly to infinity. And the result is not merely a lack of correspondence between the sound-wave and the electric impulse; for, as instruments are generally arranged, another phenomenon takes place at this stage. The sudden complete parting of the electrodes is followed by a spark due to the self-induction of the circuit; and the spark, being a sudden electric discharge of excessively short duration, produces a harsh and loud noise in the receiver. Many persons call this noise the noise of make and break. It is nothing of the sort. Making of the circuit and breaking of the circuit can be accomplished without any

such disturbance, provided the extra-current sparks are suppressed. Another point is less easily stated. All the transmitters in which carbon contacts are used are defective in their transmission of certain sounds, particularly the sibilants and shrill whistling sounds. At one time I was of the opinion that this circumstance was not the fault of the transmitter at all, but arose from the receiver, which, in consequence of its self-induction, "throttled" (to use Lord Rayleigh's term) the more rapid fluctuations of the electric current. I was undeceived in this view by finding substances other than carbon which, when used for contact-points in the transmitter, enabled me, while still using the existing receivers, to transmit the sibilants with extraordinary crispness. Lastly, none of the transmitters at the date in question, save the Hunnings and the Gower-Bell, were as loud as was desirable. All the others required a very sensitive receiver, with the necessary result that the receiver received too much—was so sensitive as to be subject to the intrusion of all sorts of stray electric impulses, tending to drown its own small voice.

At that date the chief transmitters known were (1) the Blake, with its two contact-surfaces of good conducting matter, usually platinum and hard carbon, liable to spark at the break, difficult therefore to use in factories where there was vibration, and spoiled by being too loudly spoken to; (2) the Edison, with its button, or cushion, of semi-conducting compressible metallised fibre or mass of lamp-black, screwed up so that it could not spark; (3) the many varieties of Hughes's microphonic pencils, mounted on sounding boxes in all sorts of ways—Johnson's transmitter, having two of Hughes's pencils, Theiler's having three, Crossley's four, Wollaston's ("Gower-Bell") six or eight, and Ader's twelve; (4) the Hunnings's transmitter, having a loose layer of granules of coke—a form, by the way, which has lately been revived in the States with much *éclat* for the purpose of long-distance speaking, but without any single feature of novelty beyond what was known here in 1882.

The problem was to find a form of instrument which, while admitting of more powerful currents being used than was possible

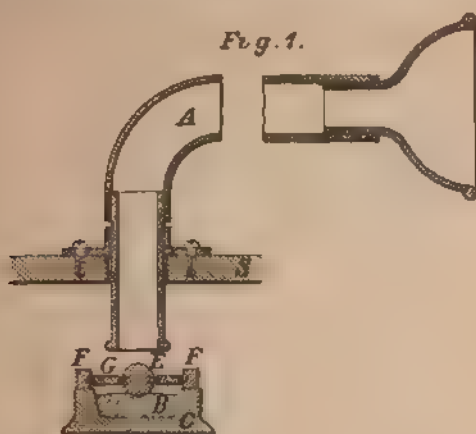


with any of these forms, would neither spark at the contacts, nor necessarily require the intervention of a diaphragm to collect the sound-waves. I am far from asserting that the presence of the diaphragm—and I use the word in the undistorted sense in which it was used ten years ago—is in all cases a detriment to a transmitter. On the contrary, in many cases it is extremely important to have one, in order to concentrate on a mechanism that otherwise would be inoperative the force derived from the impact of the sound-waves over a considerable area. I make use of this feature of construction in one of my most recent forms of instrument. There are other forms of instrument without any diaphragm, and which are better without any.

The transmitters that I have the honour of describing to-night to the Society do not pretend in any individual form to present a complete solution to the complex problem of finding the perfect telephone. Of many hundreds of forms which have successively been essayed, I will describe only a few.

From the days of Bourseul's vague suggestions and Reis's early instruments, that acoustic organ the diaphragm had been deemed a fundamental feature, until Hughes showed its non-necessity. Yeates had used it, so had Varley, so had Van der Weyde. Messrs. C. and L. Wray had used a double diaphragm. Gray, Graham Bell, Berliner, Siemens, and Edison had all made it the starting point. The discovery that no diaphragm was necessary was proclaimed by Hughes and claimed by Edison early in 1878. But, when one tried to construct practical microphone transmitters without any diaphragm to collect and concentrate the air-waves, it became apparent that the diaphragm had also served another function, namely, to intercept the moisture of the breath, which otherwise would condense on the electrodes and impede their action. Hence, in omitting the diaphragm in order to secure a more direct correspondence between the voice-waves and the electric impulses, it became necessary to devise means for getting rid of moisture. A short length of speaking-tube furnished all that was wanted. Many experiments required, however, to be made before the best form of microphonic arrangement to suit this method of working was found. One early form of tube-

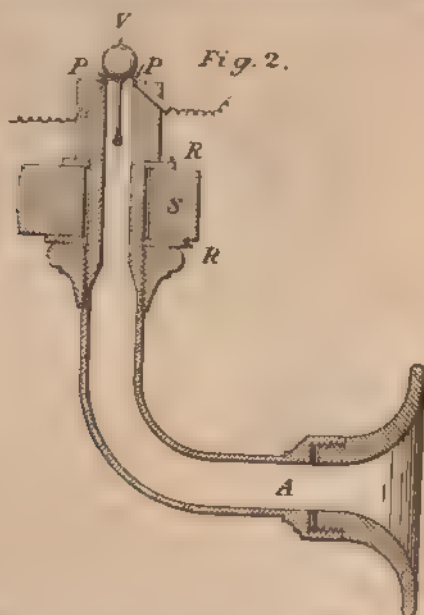
telephone on this plan had springy tongues of carbonised paste-board to constitute the contact-electrodes; another had a small pendulum arrangement like the electrodes of the Blake telephone.



In the spring of 1884 I devised the form shown in Fig. 1, to which at the time I gave the name of "Nest" telephone, because in it the working parts, instead of being mounted on springs, reposed in a sort of nest made of cotton-wool or slag-wool. Some nests had in them contact-pieces shaped like eggs; others were coke granules; others, like the figure, had a central ball, *E*, with small microphonic pencils, *G*, around it, the ball resting on the bed of wool, *B*, in the nest, and the air waves impinging through the speaking-tube, *A*, from above.

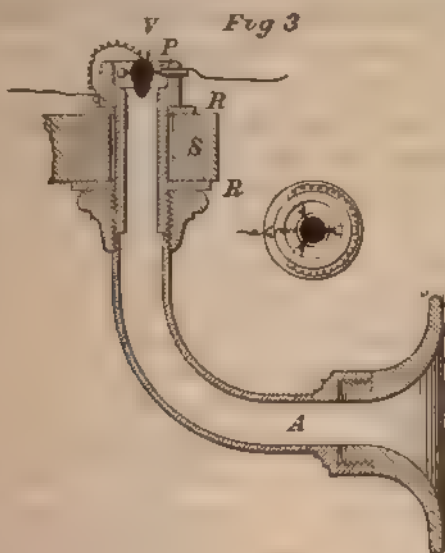
About the same time it was my fortune to have as my collaborator Mr. Philip Jolin, engineer, of Bristol, and in conjunction with him I devised the form next shown in Fig. 2. It occurred to us that if the air waves could bend a flat stiff diaphragm they might be able to lift a valve, which, though incapable of bending or vibrating in itself, might follow or ride upon the vibrations of the air-waves. After a trial of many intermediate forms, some of which are on the table before you, we settled upon the pattern shown in Fig. 2, having a polished valve-ball, *V*, reposing lightly upon three metal pins, *P*, between which it made electric contact. The speaking-tube, *A*, led the voice-waves to impinge on the under

side of the valve, and the tube itself was clamped in a supporting bracket, S, between two rings, R, of india-rubber, to prevent any stray vibrations from reaching the sensitive mechanism from the



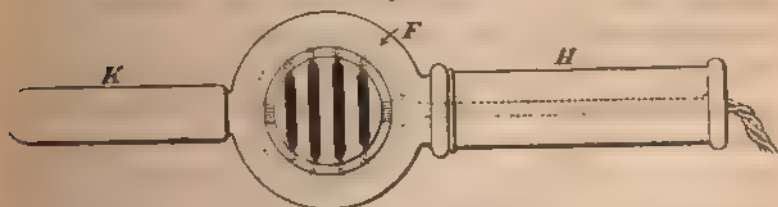
wall against which it was fixed. Concerning the material of the valve itself I will presently speak. We found that valves of many kinds—cone valves and clack valves, as well as ball valves—gave excellent results. Later, it was found advisable to modify the “Valve” telephone, and to give it the form shown in Fig. 3, in which the valve is egg-shaped, or even pegtop-shaped, rather than spherical, and the current is led into it through a fine coiled wire; the three contact-pins (of hard Carré carbon) being united so that the three contact surfaces were all in parallel with one another. With this form stronger currents could be used. In the search after the best form to give to the instrument, I was aided by Mr. John S. Sellon, who also produced several excellent transmitters having microphones of special form. In one of these a perforated ball, or bead, was slipped over the ends of two nearly-meeting cylinders, which projected horizontally over the top of the speaking-tube.

The loudness of the Valve telephone is limited by the fact that it is not expedient to work with more than three contact-points, the limit being a natural geometrical one familiar to every



constructor. When, therefore, still louder instruments are desired, either several separate valves must be combined, or some other form of instrument must be employed admitting of a larger number of contacts being introduced. A form of instrument which answers extremely well, and is both simple and handy, is

Fig. 4.

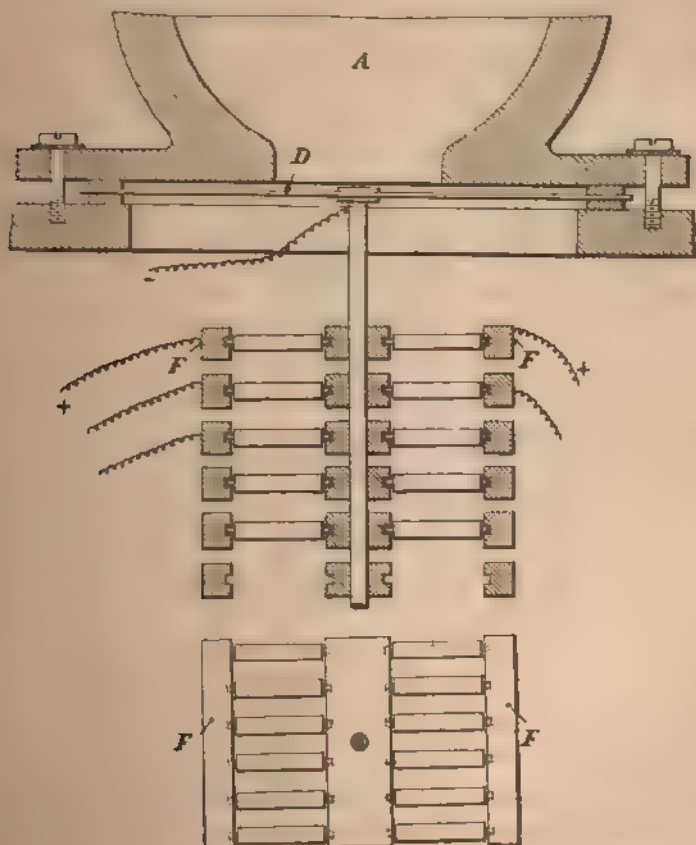


depicted in Fig. 4. It consists of a *grid* or row of microphonic pencils—Hughes's microphones pure and simple—mounted in an open frame, F, and provided with a convenient rubber-covered handle, H, through which the conducting wires pass. There are several ways of using the "Grid" telephone—it may be mounted

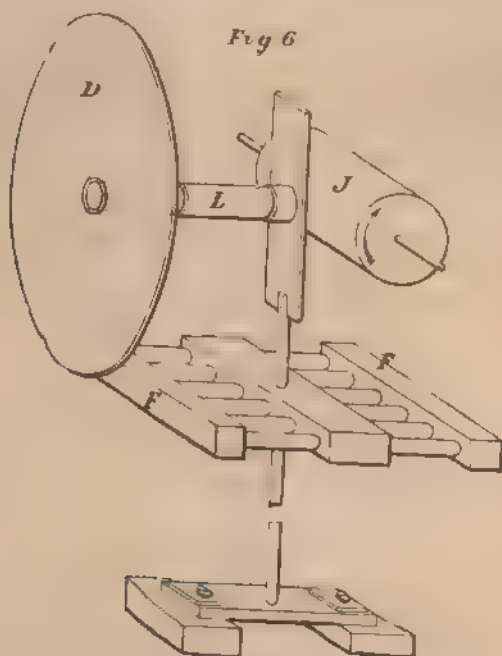
above the call-bell with the handle vertical, or it may be held in the hand and simply talked to, the voice-waves striking direct upon the microphonic pencils. But there is a third way in which it may be used, namely, that suggested so far back as 1878 by our esteemed Past-President Mr. Preece, in this very room, when he showed that Hughes's microphone required no air-waves to operate it, but might be placed against the forehead, face, or breast. About the same time Professor McKendrick found that the mechanical vibrations of the throat could work a Hughes's microphone direct. The "Grid" telephone has a second handle, K, which can very conveniently be laid against the throat or breast, and it makes an admirable transmitter. I am, indeed, told that a Mr. James Lowth, of Chicago, has brought out a whole system of telephony based on this method.

Not being quite satisfied with any of the instruments which I have now described, though any one of them is quite equal to the average Blake telephone, and some better, I have designed yet another form of instrument specially for the work of long-range transmission, or where very unusually loud effects are wanted. For long I have maintained that the clue to long-range telephony is to use much more powerful transmitters and much less sensitive receivers. The more powerful transmitter will require strong currents to pass through it in order that it may produce strong variations of current. But strong currents—currents of from  $\frac{1}{16}$  of an ampère to one or two ampères—are out of the question where only one or two or even six contact-points are used. Too strong a current through any one contact-point ruins its working. We know, both from the researches of Mr. Bidwell, from the theory of Dr. Moser, and from the experience of Wollaston and Ader transmitters, that the right solution to the problem of arranging a set of microphones in the most effective way is to place them all "in parallel" with one another, so that the main current divides itself between all the contacts. If, however, a set of microphonic pencils are placed simply all in parallel, there occurs a practical difficulty, namely, that the least sensitive microphone is that which will receive the greatest current; and, moreover, if the attempt is further made to provide the set of

microphones with electricity at a constant potential, then there is a continual liability for one contact to become overheated and spoiled. The ordinary way—for example, in the Crossley and Ader telephones—of arranging microphones is to attach them side by side beneath the under surface of a sounding-board. This arrangement seems to be open to the objection that the various parts of the sounding-board do not move simultaneously in the same direction: some are heaving up at the moment when other parts are moving down. The wonder is that such a transmitter works as well as it does. The transmitter that I have constructed for this special work has many microphones—108 in all, arranged in 54 parallels, the pencils being two in series. They can also be

*Fig. 5.*

arranged all in parallel if desired. As Fig. 5 shows, they are grouped in multiple-grids. Each grid or layer contains 12 microphones, and the motion is communicated to them by a central vertical rod which itself receives its motion from the centre of a mica diaphragm. The impulses do not, of course, pass down the steel rod absolutely instantaneously, but the retardation of phase of even the shrillest of vocal sounds is quite negligibly small. The microphones practically act simultaneously. To each layer of microphones a small resistance-coil is added, thus assimilating the practice to that adopted when a set of arc lamps are to be used in parallel. Perhaps it may be worth while to provide a small resistance to each pair of pencils hereafter. When there are so many microphones to be carried, although the voice-waves are concentrated by a diaphragm of five inches in diameter, the amplitude of vibration imparted is not very great, probably not



much greater than would be imparted by the voice acting upon the microphones direct if the diaphragm were removed. This



mode of using the transmitter is, by the way, quite successful. But if the number were further increased, the amplitude of motion imparted by the diaphragm would be insufficient. Hence I have sought other ways of obtaining mechanically the requisite energy, leaving the voice the work of controlling the motions. A method of doing this, which promises good results, was suggested by Edison's "motographic" receiver. Against a slowly-rotating friction-roller presses an extension of the rod that supports the central bars of the layers of microphones, and agitates them with a drag and slip motion. The voice is made to affect the pressure upon the slipping surfaces, and so control the periods of drag and slip. The crude notion is represented in Fig. 6. In experimenting in this direction several points of interest in the theory of microphonic contact presented themselves, and are worthy of attention.

#### EFFECTS OF HEAT ON MICROPHONIC CONTACTS.

First of these points was the effect of heat on microphonic contacts. It is a matter of common knowledge that microphonic contacts work more satisfactorily when warm than when cold. Hunnings' transmitters of granulated coke work best when heated by the current. Ochorowicz has produced a special telephone transmitter—the details of which are still kept from publication—in which a preliminary heating is brought about by using a powerful current. I have tried various arrangements of microphones in which heat was led from a small gas jet by copper rods or tubes to heat the working contacts, but have not arrived at any final form. At one time I was under the impression that the heat served simply or chiefly to increase the energy of the vibrations imparted by the voice. But further experiment showed that this was at least a crude view, for I found that if one of the two pieces in contact was heated, the microphonic joint became more sensitive to sound when the other was kept cool. In fact the actual transfer of heat across the loose contact of the circuit seems to facilitate its action as a telephonic transmitter. In studying this point I came upon another fact, namely, that the layer of vapour which exists between a drop of acidulated water



in the so-called spheroidal state and the hot metal plate beneath it will also act as a microphonic transmitter, though from its high resistance and instability it is a rather unmanageable sort of transmitter. Now we know from the researches of Dr. G. Johnson Stoney, Professor G. F. Fitzgerald, Professor Barrett, and Mr. R. Moss, that the layer of vapour which supports the spheroid is in the state of so-called "heat-polarisation," that is to say, having a peculiar stress in it transversely to the layer. The researches of this group of distinguished scientific men have shown that there is a distinct relation between the stress across the layer and the difference of temperature between its surfaces. Between these layers heat is passing by a process, termed "penetration" by Dr. Stoney, the essence of which is that the molecules which are travelling across the layer from the hot to the cold side are moving faster than those which are travelling in the contrary direction. That such a layer can conduct an electric current proves that the molecules in their flights across the layer carry electric charges upon them, exactly as do the flying molecules in the highly attenuated vapour of a vacuum tube. It is indeed significant that our Vice-President, Mr. Crookes, himself suggested, so far back as 1874, that the phenomena of the "spheroidal state" were due to the same causes as those he was studying in the repulsion which he found to exist between two bodies one of which is hotter than the other, as in the case of the blackened vanes of the radiometer and its enclosing bulb. If this view be correct, the apparent resistance of a layer of flying particles ought to be proportional—*ceteris paribus*—to the number of molecular impacts per second, and this in turn will be proportional to the area of the layer, and inversely proportional to its thickness. It is possible that it may be a novel idea to some members of the Society that if an insulated conductor of a given size is flying backwards and forwards in a regular periodic manner between two surfaces, one of which is at a higher electric potential than the other, it will act as a conductor of measurable apparent resistance. An ordinary electric bell while ringing possesses a measurable apparent resistance quite different from its resistance when standing still, and dependent on the frequency

of its vibrations and on the length of path at the contacts, as well as on the nature of the contacts. Maxwell suggested, and Mr. Glazebrook has used, a method for measuring the capacity of a condenser by measuring the apparent resistance of a vibrating contact apparatus, an electrically maintained tuning fork of known pitch. There is then good reason to think that a layer containing hundreds of thousands or even millions of molecules flying to and fro between its bounding surfaces will have a measurable resistance, even though the whole phenomenon be one of minute discontinuous discharges. Suppose that an interrupter like the hammer of an electric bell or the rheotome of an ordinary induction-coil could be made to vibrate at a pitch higher than the highest audible sound,—say, at 45,000 vibrations per second,—we might measure its resistance without being able in the least to tell that it was not an ordinary resistance. It would probably vary considerably with pressure. But here we come to the question whether the entire phenomenon of elasticity is not due to incessant minute motions; a question too wide to discuss here, and itself only part of a still wider question whether all potential energy, so-called, is not really kinetic energy after all.

Returning from this point to the more familiar one of the cause of the action between the two surfaces of a microphonic joint, let it be observed that there is an obvious incompleteness about both of the rough-and-ready theories which are commonly accepted; viz., that microphonic action is due (1) to the variation of resistance of the material under pressure, and (2) to the variation of the number of points of contact between the surfaces. The first view is known to be wrong; but is not the second, as thus stated, terribly imperfect? Little as we know yet about the exact kind of motion which constitutes heat in a body, we know at least this, that the heat in a solid body is a motion, vibratory, oscillatory, or rotatory of its particles. We know that the surface particles of every substance that is not at the absolute zero of temperature are animated incessantly with minute motions which are of an irregular kind not timed together, otherwise the body would emit rays of one frequency only, not all executed in the same

direction at the same instant, otherwise the emitted rays would be polarised rays. We know that their average amplitudes increase, and that their average frequencies also increase, with increase of temperature; but this brings us almost, if not quite, to the boundary of present knowledge. We know, further, that the most highly polished surfaces we can obtain may be, molecularly speaking, quite rough. A ridged surface whose corrugations are less than a half-millionth of an inch in breadth is perfectly flat to all optical tests, but such ridges might contain from five hundred to two thousand rows of separate molecules. A metal surface one-millionth of an inch square may have from four to sixteen thousand molecules on its top surface. Think of two surfaces such as we have to deal with in telephonic transmitters, say one of platinum and one of carbon. Both rough, both heated by the passage of the electric current, that is to say, each having its molecules in violent agitation, and ready to discharge electricity from the surface of higher to that of lower potential. Let these two be resting lightly in what we commonly call "contact:" what a jostling of molecules, what innumerable batterings, what millions of minute electric discharges; air particles, nay, even solid dust particles torn from the two surfaces, flying to and fro between them, forming a veritable Crookes's layer. Then think what is the meaning of putting more pressure on to such a "contact." The thousands of molecular makes and breaks that were going on will be multiplied; the paths of the flying molecules shortened; more current will flow; heat agitations become still more energetic. Take off the pressure, the two surfaces fling one another apart, or bombard one another apart; discharge takes place more slowly; the apparent conductivity of the layer is reduced. Move the surfaces still further apart, the atomic clash and atomic bombardment is still further enfeebled. Separate them by a visible distance,—like the spheroidal drop separated visibly from the hot plate beneath it,—still there are the flying molecules at work between them. Will any one undertake to say that at sensible distances, such as one-thousandth of an inch, or even one-hundredth of an inch apart, no discharge takes place at all across the gap? Think of this, and

then meditate on the shallowness of the dictum that when two clean platinum contacts are used in a transmitter, the current must be either wholly on or wholly off, either "made" or "broken," with a sharp transition *per saltum* between the state when the two surfaces are in perfect contact and the state when the two surfaces are not in contact at all. Think of the utter fatuity of the dictum that a transmitter cannot transmit unless the contact substances are "semi-conductors." The more one thinks of the matter from the point of view of the molecular vibrations, the more plain is it that every transmitter that works by varying the resistance of the circuit—every one of Class I. in the table of transmitters—works by varying the number of molecular contacts, and that the most perfect transmitter will be the one that does this in the most effective correspondence with the displacements in the air caused by the speaker's voice, and will give the effect of undulation when the air particles merely undulate, and will give the effect of intermission when there is intermission between the motions.

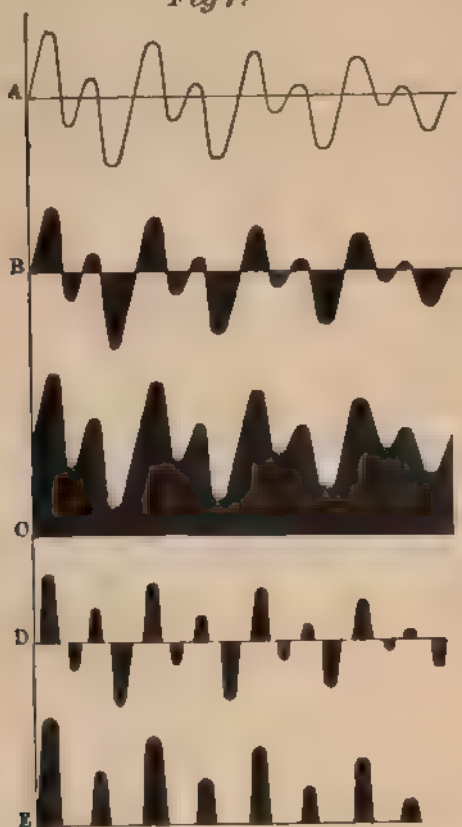
The foregoing considerations will perhaps make more intelligible the action of heat in assisting microphonic action. It is my opinion that what heat can do can also be done by auxiliary vibrations of such periods as shall not interfere with the tones of the voice. I have had an apparatus lately constructed to test this matter.

#### INTERMITTENCE IN VIBRATIONS.

It was remarked earlier in this paper that the noise commonly attributed to "make and break" is really due to sparks; and that if these are suppressed, "break" produces no more noise than "make." Under these circumstances speech is perfectly transmitted by sensitive arrangements in which otherwise the speech might be drowned by the spark-rattle. The mere fact that the vibrations corresponding to a certain sound stop for a short time does not necessarily produce on our ears any discontinuity in the sound. Nay, if, for example, the vowel "a" is being sounded in a telephone, the receiver at the other end will continue to emit the same sound, even though the electric

impulses are occasionally interrupted. Impulses, large and small, discontinuous in themselves, if rightly timed, will suffice to set up any desired form of vibration upon a proper receiving tympanum. Take a complex sound such as that represented in

*Fig 7.*



the curve A, Fig. 7. We know that if we translate this into electric undulations of the form either B or C, we shall get the same sound reproduced in the receiver. But we also get the same complex sound by the discontinuous impulses of intermittent electric currents such as are represented graphically by D and E, provided the discontinuous impulses, large and small, are rightly timed and proportioned. To assert that discontinuous electric impulses cannot set up and maintain a continuous vibration in

the armature of a telephone receiver would be as absurd as to assert that discontinuous impulses cannot set up and maintain the continuous rotation of a fly-wheel or the continuous oscillation of a swing. But it is hardly worth while to prove by argument what has been abundantly demonstrated in fact.

#### MATERIALS FOR TRANSMITTERS.

Much time has been expended by me in endeavouring to find a pair of materials which, when used as the contact-surfaces of a transmitter, should give better results than the ordinary contact of platinum against carbon. Contacts if made in both parts of metal are somewhat uncertain in action, chiefly because of the deterioration of surface. I have also examined many of the substances, natural and artificial, which possess quasi-metallic conductivity. Some of these form excellent materials, but each has its own particular qualities. Amongst natural minerals I have tried the following:—Native Arsenic, Mispickel, Ilmenite, Allemonite, Specular Iron Ore (black crystals), Magnetite, Micaceous Iron, Cloanthite, Pentlandite, Clausthalite, Redruthite, Chalcopyrites, Smaltine, and Pyrolusite. Some of these were better than others; but the best, micaceous iron, was difficult to obtain in workable pieces. Artificial preparations of copper treated with tellurium, and copper treated with selenium or with mixtures of selenium and sulphur, gave results of surprising character. A hard and brittle sort of bronze formed in this way takes a brilliant polish like steel, and yields, when used against a platinum point, an articulation which for crispness far surpasses anything to be obtained from hard carbon. At one time this bronze was used for the valve-pieces of the valve-transmitter, but although the articulation when the ball-valve was newly polished left nothing to be desired, it was found to acquire from the moisture of the air a film, probably of selenic acid, at the contacts, which gave rise to curious, shrill whistling sounds. Mr. G. L. Anders has made excellent transmitters of osmium and of palladium. Nickel also answers fairly well. There is a great difference between different materials with respect to the range of amplitude of vibrations suitable for them. For the metals in general the



range of amplitude of motion must be very small: they require almost rigid connections. With hard carbon and with the copper-selenium composition the range is much greater. The copper-selenium composition requires a greater initial pressure at the contacts than hard carbon does. So also does chalcopyrites, which has recently been suggested by Mr. P. Rabbidge. In our present state of knowledge of the properties of those bodies which possess quasi-metallic conductivity, it is difficult to frame any explanation of these facts, or draw a useful deduction from them.

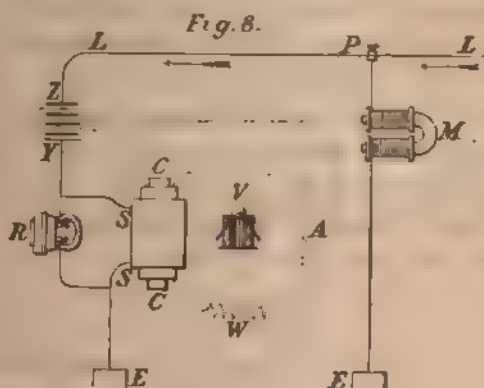
#### TRANSMITTERS ON OTHER PRINCIPLES.

Incidentally my work has touched one other method of telephonic transmission, namely, that originated by Graham Bell. To Bell belongs the unquestioned, and I think unquestionable honour of having suggested the magneto-electric method of transmitting, as distinguished from the contact method. But, in nine cases out of ten, where telephones are used, Bell's method is not employed, having been abandoned in favour of battery and variable contact. Even the Imperial German Post Office, which for eight years has been faithful to Bell's method, has now abandoned it, and is replacing all its magneto-electric transmitters by microphonic transmitters. My own work, which is more fully detailed under the heading of "receivers," consists in devising a form of instrument on the principles, and with some of the actual parts, of the modern dynamo-electric machine. I have also succeeded in transmitting speech by an apparatus the principle of which was to use a nearly constant current, but to vary the number of times that it circulated around the primary of the transformer, by cutting out more or fewer of the coils.

#### TRANSFORMERS.

The desirability of getting rid of sparks at the electrodes has been referred to. A shunt made of a thin resistance wire (as in Johnson's transmitter), or a liquid resistance, does not satisfactorily accomplish this end, though it mitigates the evil. I have found that the best method is to use a differentially wound transformer. My coadjutor, Mr. Jolin, had found, when experimenting

on an electric meter, that a double differential winding on an electro-magnet enabled him to open and close the circuit without any appreciable spark, even when using very strong currents. On suggesting to him that a similar device might be applied to the primary coils of the transformer, the transmitter being placed in one branch, he constructed one, but found it expedient to add a balancing resistance in the other branch. The arrangement is



shown in Fig. 8, in which the system of circuits is shown. The primary circuit is shown in dotted lines, dividing at A; the valve transmitter, V, being in one primary, and a thin German silver wire, W, as a resistance in the other. The secondary coil is in parallel with the receiver. The electro-magnet shown at M relates not to the transmitting circuit at all, but to a peculiar arrangement of receiving circuits, in which an electro-magnet (that of the call-bell) is used as an induction plug. It may be remarked in passing that this differential method of working is applicable to other transmitters; in particular with sensitively adjusted transmitters, to which one must not speak too loud, and which are affected by vibration of buildings, the articulation is immensely improved by its use. Even the Blake works excellently amongst the machinery of a factory when provided with one of the differential transformers. The "Valve" telephones are all provided with them. You may shout as loud as you please to these instruments without producing any unpleasant rattling sound. We also constructed transformers having closed magnetic



circuits, but found that the increased labour of manufacture outweighed the advantage. A transformer very similar to that used in the Blake telephone, but with slightly more primary wire, has been found superior to the larger transformers used by the General Post Office in their Gower-Bell telephones. In transformers for telephonic purposes, it is desirable to have such an arrangement as shall give maximum mutual induction with minimum self-induction. As induction-coils are usually constructed, it would appear that when the size is increased the mutual induction does not increase proportionally. At any rate, two small ordinary induction-coils, having their primaries united in parallel and their secondaries in series, give a better result than the same of wires made up in one single induction-coil of similar form. Possibly Mr. Langdon-Davies's phonopore may eventually prove the best transformer for telephonic work.

#### RECEIVERS.

In the course of my work many devices for the improvement of receiving instruments have been tried. To place the subject in a clearer light, I here append a systematic table of the various classes and genera of receiving instruments.

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#### CLASSIFICATION OF RECEIVERS.

##### I. *Electro-magnetic Force.*

- |  |     |     |  |
|--|-----|-----|--|
| 1. Magnetic expansion                        | ... | ... | Reis.  |
| 2. Electro-magnet with armature              | ... |     | Reis, Yeates, Varley,<br>Gray, Bell, Mac-<br>donough, etc. |
| 3. Two electro-magnets attracting            |     |     | C. & L. Wray.  |
| 4. Electro-magnet itself vibrating...        |     |     | Theiler.   |
| 5. Electro-magnet altering friction          |     |     | Dolbear, Cooke.  |
| 6. Solenoid and magnet                       | ... | ... | Bell, Siemens.   |
| 7. Two coils attracting or repelling         |     |     | Vail, Taylor.  |
| 8. Deflexion of needle by coil               | ... |     | Bidwell.   |
| 9. Pivoted armature near field-<br>magnet .. | ... | ... | ...  |

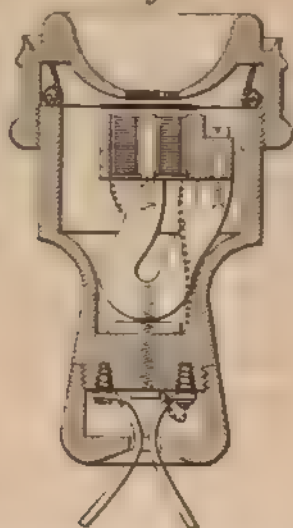
CLASSIFICATION OF RECEIVERS—*continued*.

10. Magnetic repulsion	...	...	
11. Two parallel wires	...	...	
II. <i>Electrostatic Force.</i>			
1. Air-condenser attraction	...	Wright, Varley, Dolbear.	
2. Air-sound from alteration of surface-density of charge...	...	?	
3. Electrostatic repulsion	...	...	
III. <i>Electro-capillary Force</i>	...	...	A. Breguet.
IV. <i>Tribo-electric Force.</i>			
1. Physiological (increase of friction)		Gray.	
2. Moist chalk against metal (decrease of friction)	...	...	Edison.
V. <i>Microphonic Contact Repulsion</i>	...	Berliner, Hughes.	
VI. <i>Thermal Expansion.</i>			
1. Expansion of wire by heat	...	Preece.	
2. Glow-lamp	...	...	Dolbear.
VII. <i>Crookes' Tube</i>	...	...	Dolbear.
VIII. <i>Crookes' Layer</i>	...	...	

With the exception of the discovery that a Crookes' layer between a "spheroidal" drop and the plate below it can act as a receiver, and that a receiver can be made to work (feebly) on the principle of electrostatic repulsion, the only work that I have to mention relates to receivers of the first class—those in which the electric current produces the motion by means of the magnetic force which it exerts. Upon the table here I have several instruments based on the principle of magnetic expansion. They all require stronger currents than the ordinary receivers. Messrs. Alabaster and Gatehouse have worked in this direction also with some success. Next I exhibit a modification of Bell's membrane electro-magnetic receiver, in which the form given to the electro-magnet is the one found to be the best for the attraction of the small disc-armature, about the size of the thumb-nail. The coil is surrounded (Fig. 9) by a circular pole-piece, cut away at one point to hinder eddy-currents; this outer pole-piece being of

opposite polarity to the central pole-piece. The pull of such an electro-magnet upon its armature is powerful, and varies greatly with the thickness of the gap between the iron disc and the poles.

*Fig 9.*



This modified form of the membranous receiver is an exceedingly satisfactory instrument. Its magnetism requires to be excited by a current while in use; but when used as I have been employing it, along with the "Valve" transmitter, it proves to be less sensitive to induction than most other instruments; and is indeed now in use on lines where, with other instruments, conversation was impossible, owing to the deluge of stray noises.

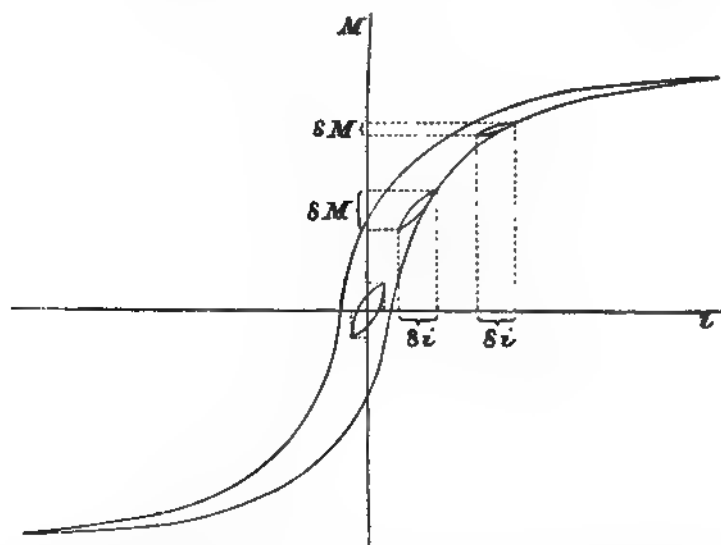
I have found that two iron wires stretched close together across a sounding-board, and traversed by the current, will answer as a receiver; but their natural note interferes with the tones of the voice. A receiver can also be made in which the working parts are two parallel iron cores which repel each other when similarly magnetised.

But my attention has been more directed of late to the object of bringing to bear on the small scale the same general principles which have been found to hold good on the larger scale in the construction of dynamo-machines and motors. Given a circuit

into which the transmitting apparatus has thrown fluctuations of electro-motive force of a certain amplitude, the problem is to utilise in the receiver the largest amount of the electric energy. All the principles that come into play in the electric transmission of energy come in here, but with certain restrictions. For example, not only must a certain amount of electric energy be transformed into mechanical energy, in order to give the sound of the required loudness, but the mechanism must be such that the organ which emits the sounds, be it a tongue or a tympanum, or any other organ, must be capable of moving with large amplitudes or small, with rapid frequencies or slow, and must still in moving generate such counter-electro-motive forces as shall make the work utilised a maximum. All such principles of construction as apply in dynamos to the avoidance of eddy-currents, of needless self-induction, of magnetic leakage, and the like, also apply to the receiving telephone. There is a conductor which carries a current, there is a magnetic field in which the conductor lies. Either the conductor, or else the field-magnet or some part of it, must move, and it is desired to make their mutual reaction as great as possible. If the coil is to move it must be placed in the strongest possible field, and so placed that its minimum motion shall alter by a maximum amount the total magnetic induction through it. The iron or steel should be arranged so that the magnetic circuit which they form should be as nearly closed on itself as possible. By the light of dynamo principles it is simply absurd to construct a receiver in which one pole of the magnet is far away from the working parts. Furthermore, just as in every dynamo and motor there is a gap or gaps in the magnetic circuit where the working coils are situated, so in telephonic receivers there are gaps in the magnetic circuits, and it is there that the working coils should be situated. Take any one of the modern improved receivers—those of Siemens, Gower, Ader, d'Arsonval, Ochorowicz—their magnetic circuit is all but closed, and the coils are *at* the gaps. It is there that their currents in passing through a given fluctuation will produce the greatest effect on the magnetism of the magnetic circuit, and therefore on the attraction between the two parts. The current must be brought to bear, so to speak, upon a loose-joint in the *magnetic circuit*.

There is another matter in which our present knowledge is very imperfect. It is found that in every receiver (even the electrostatic ones too) there must be an initial force exerted upon the vibrating part, be it tongue or tympanum, or any other kind. A mere unexcited electro-magnet placed behind an iron diaphragm is practically useless to receive alternating currents from the secondary coil of a transformer. It does not speak out until its core has received an initial magnetism either from an initial current or from an auxiliary magnet, permanent or temporary. No doubt a portion of this effect is purely mechanical, for the same reason that an acoustic telephone does not speak properly unless the line between its two tympanums is taut. Moreover, as Mr. Giltay has shown, a non-polarised core, excited by an alternating current, would give rise to double the proper number of vibrations—would raise the pitch an octave. But there is more than this, and it requires further investigation. Consider the diagram (Fig. 10), which represents a cycle of magnetising

Fig. 10.



operations such as we have become familiar with from the researches of Dr. J. Hopkinson and of Professor Ewing. We know that if we first of all decrease the magnetising current by a certain

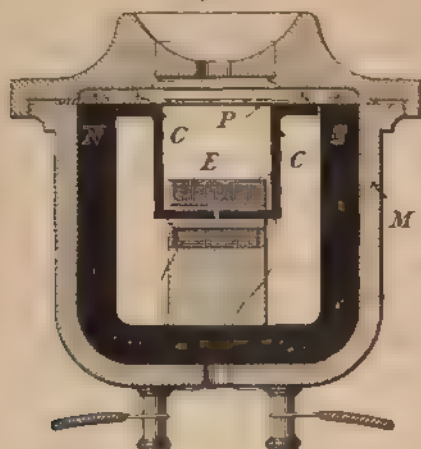
amount,  $\delta i$ , and then increase it again by an equal amount, we shall carry the magnetism down and up again along a small cycle. Three complete small cycles are represented in the figure. Now one would have expected that one would get the greatest variation in magnetism for a given variation of current at that part of the diagram where the curves are steepest, where the ratio  $\delta M / \delta i$  is greatest. That is to say, one would expect the iron core to be most sensitive to current fluctuations when there was no initial magnetism, and least sensitive when there was great initial magnetisation. But experience, not with telephone receiver only, but with polarised relays, tells us that this is not true where time comes into account, and that if we make the current fluctuate *rapidly*, we get the greatest magnetic fluctuations when there is some initial magnetisation. Some experiments of mine now in progress go to show that the "sensitiveness" of the iron core increases with an increase of initial magnetisation, at first slowly, then more rapidly, and has not attained a maximum even though the magnetisation be pushed far beyond the bend of the curve of magnetisation. But there are some obscure contradictory facts still requiring explanation. In view of the fact that an initial magnetisation is needful to make a receiver respond properly, it appears to me that an absurd amount of importance has been attached to the disputed discovery that a permanent magnet might provide this initial magnetisation instead of a permanent current. No doubt it was surprising to learn that a transmitter could transmit without employing a battery; but as this method of transmitting has been practically abandoned, the importance of the discovery vanishes with it. There is no novelty whatever in using a mere polarised receiver.

It now only remains for me to describe briefly three forms of receiving instruments in which some of these ideas are embodied. Examples of most of them are on the table, and they are illustrated by the remaining figures.

Fig. 11 shows a form of instrument named a "Spring" receiver. It is based upon the early form of C. and L. Wray, in which two electro-magnet cores attract one another, and so impart motion to a sounding-board. In my instrument the

sounding-board is replaced by a flat spring, P, of mica, ebonite, or some non-magnetic metal, to which are attached the two working cores C, C. These form part of the magnetic circuit of

*Fig 11*



an external magnet, N S, and the working coil, E, is placed over the gap in the magnetic circuit. Iron or steel, if used for the spring, would spoil the action, as it would short-circuit the magnetism. This receiver works extremely well on induction circuits. An important point in its design is that the centre of the spring, which will have the greatest amplitude of motion and is most active in imparting sound to the air, is not under a perpetual magnetic constraint like the central part of the iron diaphragm of the common telephone receiver.

*Fig. 12 p*

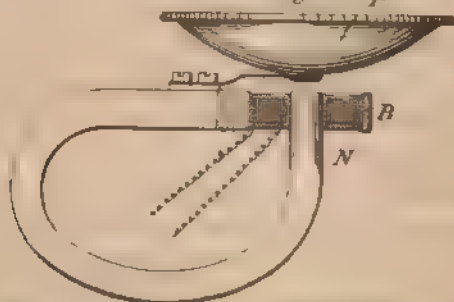


Fig. 12 illustrates a form of receiver in which the magnet is



a long narrow triangle of steel, having the apex bent round and inserted in a circular cavity cut out in the base of the triangle. The south pole thus surrounds the north, and in the intervening gap lies the working coil. Instead of the usual iron plate held down all round, there is a metal cup, or dish of metal, ebonite or celluloid, which can be held over the ear, and it is held up at its centre by being riveted by a stout iron rivet to a short triangular steel spring. This instrument, even in a crude form, is excellent both as receiver and transmitter.

Fig 13

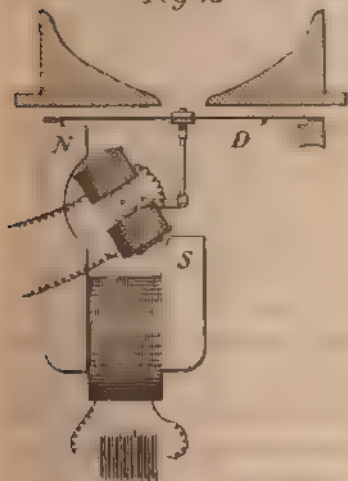
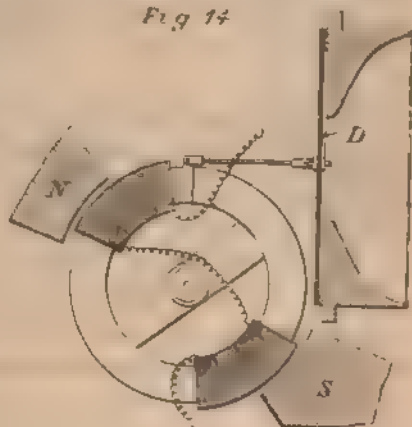


Fig 14



Figures 13 and 14 relate to a form of instrument which I have ventured to term a "*Dynamo-telephone*," inasmuch as the action of these instruments is directly taken from that of the modern dynamo. The parts are distinctly differentiated into a field-magnet, which stands still and furnishes an intense magnetic field, and an armature whose coils are traversed by the working currents. The main point of difference is that, whereas in the dynamo-electric machine, whether used as generator or as motor, the armature rotates, in the dynamo-telephone, whether used as transmitter or receiver, the armature merely vibrates, being pivoted elastically. It is represented in the drawings as being connected to a diaphragm, D. Of other forms of instrument which are not yet out of the laboratory stage, I must not speak.



In conclusion, I would reiterate my conviction that the success of long-range telephony depends upon the possibility of devising instruments which, on the one hand, can be used with higher battery power to transmit stronger currents, and which, on the other hand, will be adapted to receive these currents by means of apparatus which, though not necessarily more sensitive to small currents than the present receivers, will have a higher electrical and mechanical efficiency. And I am convinced that the path of progress lies very near the road already travelled by those who have perfected the existing machinery for the electric transmission of power.

The  
President

The PRESIDENT: There is still time left for some discussion upon this interesting paper, and I should like to hear any views which may be expressed. Mr. Preece, have you anything to suggest upon the paper?

Mr. Preece.

Mr. W. H. PREECE: I was in very sincere hopes that the minute hand of the clock would have progressed a little further, so that the discussion might be postponed to the next meeting. I have a good deal to say on this subject, and a good deal to say, I am afraid, that will not be very flattering or altogether satisfactory to Professor Silvanus Thompson. But he has himself rather courted that "pure atmosphere of criticism" which we all breathe in this room, and if I do find fault with what he says, I hope he will attribute to me the very sincerest wishes to put him on the right path, and to enable him to surmount what he has been trying for some years with so much energy to do. I think it right to say at once that I utterly and totally dissent from nearly all his conclusions. He says that his conviction is that the question of long-range telephony is to be solved by obtaining the means or power to transmit stronger currents, and to use receivers of less delicacy, than those in use at the present moment. Now, I have said in this room before, and as Professor Thompson has reiterated his conviction, I will, in the very strongest language that I can use, reiterate mine—not as a matter of opinion, but as a matter of fact—that the difficulty in speaking to long distances is not a question of apparatus at all: it is

simply a question of the environment of the wire. On the last Mr. Prosser. occasion when the subject of long-distance telephony was discussed in this room, Mr. Van Rysselberghe himself stated that he had succeeded in talking between New York and Chicago, a distance of nearly 1,000 miles, on a line composed of a thick copper wire. On that occasion I narrated in this room a great many experiments that were made in long-distance speaking. For the last ten years I have been incessantly experimenting in this direction. There is not a line in this country upon which I have not tried to speak; there is not an instrument that has been brought to this country that I have not tried, or scarcely any one (those shown to-night I have not seen before); but the result of my experience on long lines, in America, in England, and on the Continent, is, that long-distance speaking can only be attained when a line is free from electrostatic and electromagnetic induction. It happens that only within the last few days (I think this day week) I was at Worcester. Through that town there passes a new line of telegraph, consisting of four copper wires weighing each 150 lbs. to the mile, giving an average resistance of a little less than 6 ohms—I think 5.72 is the correct figure. That line entirely, excepting for about a quarter of a mile near Great Missenden and three-quarters of a mile at Aylesbury, is separated from any other telegraphic line, and the result is that between London and Nevin, on the north-west coast of Wales, we have a line 270 miles long through which we can speak with far more distinctness and far more loudness and clearness than we can between London and Westminster. I was at Worcester; at Nevin there was one of our officers, Mr. M. Couper; and at Hanwell, near London, Mr. W. Brown; and, although we were separated by a distance of over 100 miles on the one hand, and over 160 miles on the other, we three carried on conversation and spoke to each other as clearly and distinctly as though we had been in the same room. The sibilant sounds, to which Professor Thompson referred, came out absolutely and beautifully clear, and whistling was simply transmitted to perfection; in fact, it seemed as if Mr. Couper's whistling at Nevin, nearly 100 miles off, came from the box in which I was listening.

Mr. Preese.

That experiment showed that, given a line free from induction and free from disturbances, the commonest instrument that has been produced would speak as well on a long as on a short circuit; and, in fact, last Saturday morning I repeated the experiment from London with the very original Bell telephone that I brought over with me from America in 1877,—the original box form described in Bell's patent,—and with that instrument I was able to speak as effectively as with any other. It was thus shown unmistakably that the clearness of speech which was conducted was simply due to the absence of any disturbance; for the moment the wires were put on to any other line, only a quarter-mile or half-mile in length, where disturbances became evident, then speech was impossible, and it was immaterial on which wires we tried to speak—all were alike. It was perfectly evident that, while when everything was clear, speech was possible, if any one wire on the line had been used for telegraphic purposes, speech would have broken down at once. Therefore, I say, from that experiment, and from thousands and thousands of others, that the only difficulty to long-distance speaking is that of the environment of the wires, not of the apparatus itself.

The next objection that I raise to Professor Thompson's paper, is that he rather carries too far that which, for want of a better name, I call the perversion of history. I think it is unjust and ungenerous to bracket in the same category the names of Varley and C. and L. Wray with those of Graham Bell and Edison. The apparatus of Reiss, of Varley, and of Wray was intimately known to all those who took interest in telegraphy. It was exhibited at our soirées. The Reiss telephone was shown at a soirée we held, I think, at South Kensington, and it was shown at the Loan Exhibition of 1876; and in 1876 there was not a single man in England who had heard of the possibility or the practicability of transmitting speech through a wire, or even believed that it was possible to do so. Sir William Thomson brought the first news over to England. Rumour reached here that a young Scotchman had succeeded in transmitting the voice to a distance of 16 or 17 miles, but nobody believed it; and I was so sceptical about it myself, that I went over to America in the early part of 1877,

determined to expose the fraud that was being, as I imagined, perpetrated upon the scientific community. I made an appointment with Professor Bell at the Fifth Avenue Hotel, and I went there believing that in ten minutes I should see the fallacy of the thing; but in five minutes, half the time, Professor Bell convinced me that he had transmitted speech, and that he had made a real and true invention. Therefore, seeing that the practical speaking telephone was produced by the exertions of Professor Bell, Mr. Edison, and Professor Hughes (who is here to protect himself, and I am sure he will), I say that it is unfair, ungenerous, and unjust to pervert history by classifying men who have made such a tremendous stride with men who only played with a toy.

Again, Mr. Van Rysselberghe has been roughly handled by Professor Thompson, who classified him with Black and Rosebrugh in his efforts to obtain long-distance speaking. It was not to obtain long-distance speaking that Van Rysselberghe took up this subject, but by his work, which is deserving of approbation, he showed how it was possible so to tone down the disturbances on telephones arising from induction between wire and wire as to render it possible to utilise telegraph wires for telephonic purposes. His invention was a mode by which it was possible to make use of two wires between Brussels and Antwerp for telephone as well as telegraph purposes, and I say that Van Rysselberghe made a great invention: he brought it before us here, in a way that we all admired very much, and the least we can do is to express our ideas of the amount of credit that is due to him.

I hope soon to say something pleasant to Professor Thompson, but there is yet another thing in which I must find a little fault, and that is in a direction in which he is not the only culprit—he is so fond of introducing new terms. Every man in this room knows that an induction-coil is part and parcel of telephonic apparatus, and why on earth, then, does Professor Thompson call it a transformer? The term “transformer” was introduced by Zupernowski to express that which other people call a secondary generator. It never to my knowledge has before been used for a telephone—“induction-coil” is a very much better term. But Captain Cardew, who will probably be here on the next occasion,

Mr. Preoce. will show us how he succeeded in dispensing with a "transformer" altogether. I have been trying some experiments for him. I will not detract from the merit that may be due to him, he shall bring the subject before us himself; or, if he is not here on the next occasion, he shall write a paper, or I will do it for him, and we will see how it is possible to dispense with the transformer in its present form.

There is another part of the paper in which Professor Thompson used poetic language, and said, "Think of the utter fatuity of the dictum that a transmitter cannot transmit unless the contact substances are 'semi-conductors.'" He is not pleased with the term "semi-conductor," but what does he call it a little further on?—a "quasi-metallic conductor." What is the difference between a semi-conductor and a quasi-metallic conductor? A semi-conductor is something we know and understand very well, and I think we shall continue to use it, although perhaps it is not the very best term that might have been adopted.

I will not individualise Professor Thompson, but I will bring within one broad stricture nearly everybody who attempts to make improvements, not only in telephonic apparatus but in all other apparatus. They raise imaginary difficulties, and then devise the means of overcoming them. In all that has been said about the transmitters before us this evening, I have not listened to one single sentence that I regard as true. The objections that are raised to the carbon transmitters are imaginary objections, and what has Professor Thompson himself done but come back to a carbon microphone? In his valve telephone and in that transmitter which is to contain so many hundred or thousand pieces of carbon, nothing else exists but a microphone in some shape or another. The problem that Professor Thompson put before himself to solve, was not the removal of imaginary difficulties in the working of telephonic apparatus, but to avoid the patents held by the United Telephone Company. I wish he had succeeded. I wish anybody could succeed. I am, as you all know, no great believer in a monopoly of any sort or kind. Some members laugh at this remark, because no doubt they look upon the Government control of the telegraphs in this country as a

monopoly. It is not, it never was, and it never will be ; anybody Mr. Procs.  
can invent his own telegraph, and if it is a good one it will be  
adopted and paid for. But, unfortunately, in this country the  
telephone has become a rather unpleasant monopoly. However,  
that is not the question, it is this—when a man by scientific  
work, by labour, or perhaps by accident, succeeds in bringing out  
a good thing and in getting it patented, it becomes his own  
property; and a man who deliberately sets to work to acquire that  
property does almost exactly the same thing as I should do if I  
were to set to work to deliberately secure Sir Edward Guinness'  
£8,000,000 which he received for the purchase of his brewery.  
The telephone is a personal property, and I think that we who  
patent, and who place a value upon our patents, are in honour  
bound to do something to support the principle I have pointed  
out. On the other hand, if an idea only is given to a man, and  
one sees another particular way of doing the same thing, why  
then, perhaps, there is not much harm in taking a patent out for  
it, provided you pay due regard to the other man's property.  
But I do not like the persistent way in which some people set to  
work, especially in America, to try and undermine a patent that  
is really a very good property, and which will in the course of  
three or four years be everybody's property. So much for finding  
fault.

I must say a word in praise of the way in which the paper  
has been put together; but as I have made so many experiments  
in this direction, and as I have some little things that I should  
like to bring before the Society, perhaps I may be allowed to  
commence the discussion at the next meeting, as time is so short  
now, and I will not occupy the meeting more than a quarter of  
an hour.

The PRESIDENT: Then we will adjourn the continuation of this  
discussion until the next meeting, which will be held on  
February 10th.

A hearty vote of thanks was, on the motion of the President,  
accorded to Professor Thompson for his paper.

A ballot took place, at which the following were elected :—

*Foreign Members :*

Jules Carpentier.		Ludvig Jensen.
Gustave Cabanellas.		Captain G. C. Wassmann.

*Associates :*

Francis Anderson Harris.		Robert Clay Jones.
Gilbert Scott Ram.		

*Students :*

Alfred Hewson Bate.		Edward Wythe Smith.
Thomas Whittow.		

The meeting then adjourned.

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The One Hundred and Sixty-second Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 10th, 1887—Sir CHARLES T. BRIGHT, M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates for election were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council:—

From the class of Students to that of Associates—

Frank Hughe Bocquet.

Donations to the Library were announced as having been received since the last meeting from the Indian Telegraph Department; Mr. Gerhardi, Dr. J. Hopkinson, and Mr. R. von Fischer Treuenfeld, Members; to all of whom a hearty vote of thanks was accorded.

The PRESIDENT: Gentlemen,—At the meeting before last it will be remembered that a vote of condolence was proposed to the widow of our late Vice-President and Honorary Secretary, Sir Francis Bolton, and it has become our duty to elect a Vice-President in his place. That duty has been performed to-day, and I have to report that the result of the Council vote by ballot gave the selection to Professor W. E. Ayrton, F.R.S., Member of Council. To fill the vacancy among the ordinary Members of Council thus caused, a further Council ballot took place, when Professor A. W. Rucker, F.R.S., was duly elected.

My next duty is a very sad and mournful one: it is to announce that which probably some of you may not have heard of—the death (which took place on Tuesday, February 1st) of our highly valued friend and distinguished Past-President of the



Society, Lieut.-Colonel Sir John Underwood Bateman-Champain, R.E., K.C.M.G. I am sure that all those who remember the time of his presidency of the Society in 1879, when the International Telegraph Conference of delegates from every part of the civilised world was held in London, cannot forget the service which he then rendered to the Society. All who knew him—and it was my good fortune to know him intimately—will remember him as always showing the kindness and geniality of a true friend, and as one who also had a thorough appreciation of the word “duty.” It may not be generally known, but he was one of the most gallant officers even in the distinguished branch of the service to which he belonged. He entered the Bengal Engineers—since amalgamated with the Royal Engineers—in June, 1853. It was not many years before the Indian Mutiny broke out, and I can tell you that he became celebrated, like his senior officer, Colonel Patrick Stewart, and was very soon well known as an officer of the highest consideration in the service. After having served at Agra, Delhi, and Cawnpore with the very greatest distinction, he was twice thanked in General Orders by Sir Robert Napier and by General Barnard. In other words, he did his duty—did it well; and there was never a better officer or a more perfect gentleman, *sans peur et sans reproche*, than Sir J. U. Bateman-Champain. He was afterwards executive engineer at Lucknow, until ordered to Persia in 1862 as assistant to Colonel Patrick Stewart, whom he accompanied over the proposed line of land telegraph to be constructed for the Government of India through Persia. I first met him during the laying of the Persian Gulf Submarine Cable in 1864, at Bushire in Persia, and we were together a great deal. After Colonel Stewart’s lamented death in Constantinople, which occurred soon after I parted with him, Colonel Champain was appointed temporary Director in charge of the Indo-European line. Later, Sir Frederick Goldsmid was appointed Director-General. In 1873, when Sir Frederick Goldsmid was appointed by the Government to be Commissioner for the settlement of the frontier between Persia and Afghanistan, and between Persia and Beloochistan (compelling him to give up his telegraphic

appointment), Colonel Champain became the Director-General of the line. That post he continued to fill until his deplorable death at San Remo last week. It is hardly necessary to say more as to his excellent qualities and the kindness he always showed, not only to his friends, but to those under him, and I now propose—"That the President, Council, and Members of the Society of Telegraph-Engineers and Electricians desire hereby to record their profound regret at the decease of Colonel Sir John Underwood Bateman-Champain, R.E., K.C.M.G., their distinguished and much-esteemed Past-President; and they further desire to offer to Lady Bateman-Champain the expression of their deep sympathy with her in her bereavement."

Major-General C. E. WEBBER, C.B.: Having been called upon to do so by the President, I will, in a few words, second the resolution which has just been moved. There are few of us who knew him who remember Colonel Champain without great affection, and therefore the task is one which one undertakes with considerable feeling. I will only recall to your memory the night when he sat for the first time in that [*pointing to the Presidential*] chair as our President, and when, in a way which we shall never forget, he disclaimed all personal claim to the honour which had been conferred upon him. He showed through his life a modesty which was only equalled by his bravery, and he has left behind him a respect which reaches far beyond the shores of this country. He was one of those bright examples of the officers whom (we may say) we inherited from the service of the East India Company—a band of men who brought England's name to be honoured in the government of that great country. His associating his military duties as an engineer with his high civil position brought him into that place which has led to our this day deeply regretting the loss to his country and to our Society of so distinguished and honourable a man. I beg to second the proposal of our President that the hearty condolence of this Society should be conveyed to his widow and family.

The motion was carried unanimously.

The adjourned discussion on Professor Silvanus Thompson's paper on "Telephonic Investigations" was then resumed.

Mr. Preece.

Mr. W. H. PREECE: I mentioned, Sir, on the last occasion that, owing to the want of time, I was not able to bring before the Society certain experiments and evidence that would support me in my contention that Professor S. Thompson was wrong in asserting that there was any possibility of improving our mode of communication by telephone to great distances by looking to the apparatus and not by regarding the wire. The law that determines the transmission of currents through a wire to produce telephonic signals is precisely the same in every respect as the law that determines the flow of currents through submarine cables, or, indeed, through any circuit. The experimental evidence upon which this law was determined was published in the report of a joint committee appointed by the Board of Trade and by the Atlantic telegraph companies in, I think, 1860 or 1861, and the experiments were carried out by Mr. Latimer Clark, whose assistant I was at the time. I carried out all those experiments for him, and I also had for days the advantage of showing and assisting Professor Faraday himself in carrying out certain experiments of his own. Professor Faraday devoted a Friday evening at the Royal Institution to detail those experiments, and they are published in his *Researches*. These experiments formed the basis of the mathematical development of the law by Sir William Thomson—which was produced before the Royal Society in 1854, and published in 1855—who pointed out that if we regard the current coming out at the end of a cable, that current can be represented by a curve. Mr. Hockin subsequently gave a series that enabled one to draw that curve, but the curve itself was given by Sir William Thomson. If we represent time by the abscissæ, and the intensity or strength of the current by the ordinates, then a current being applied at the sending end of a cable, a certain time elapses, called  $a$ , and at the end of that time the current appears and gradually rises until, after a longer interval, it assumes a maximum; and if we divide the abscissæ into intervals—into terms of  $a$ —then at any period the fall of the current can be shown by a reverse curve. The whole law depends upon  $a$ , which is given by an equation containing a constant that depends principally upon the units used; on  $k$ , which

is the static capacity per unit length, per knot, of cable; Mr. Prescott  
by  $r$ , which is the resistance per knot; and by the square of the length of the cable. Now that law—which is known as Sir William Thomson's law—which is as true as Ohm's law, and which lies at the root of all developments of submarine cables, was proved to be true by Fleeming Jenkin, not only in its application to submarine lines, but to land lines, and, in fact, to all circuits; it was proved to be true by Cromwell Varley by a most beautiful apparatus that he devised, called the wave bisector; and it is proved true by every single cable which exists at present in the world, and which, I may say, at this moment adds up to a length of 110,000 miles; and therefore I say that the law that determines the flow of electricity through a wire—the law that determines the number of currents that can be passed through a wire in a given time—is as absolutely exact and as absolutely true as any other law that regulates any portion of electrical effects. I want to call your especial attention to one feature of this law: it is that the value  $\alpha$  is absolutely independent of the impressed electro-motive force at the sending end of the cable; it is absolutely independent of the current itself. Professor Fleeming Jenkin pointed out that there is no available method of human ingenuity that can enable us to reduce that value  $\alpha$ , or that can enable us to send a greater number of currents in a given time through a circuit than that law allows us to do. So that the number of currents that pass through a wire is not in any shape or form dependent upon the electro-motive force starting that current, nor is it in any shape or form dependent upon the strength of that current; and therefore, on the basis of the law itself, I say that it is absolutely impossible, along a given length of wire or circuit, by any alteration in the transmitter, to get any greater number of signals through.

We happened lately to have introduced in the Post Office a system of working with the Delany multiplex telegraph (it is a system based on a principle introduced by Meyer in Vienna); and by that apparatus we are able to cut up a current into any number of intervals of time, and we are able to measure with the most startling accuracy the rate at which a current of electricity flows

Mr. Prosser. through a circuit; and it is remarkable how exactly this value of  $\alpha$ —how exactly the rate at which the current flows through a wire—will give the distance to which speaking can be carried on by means of a telephone. The number of experiments in this direction that I have carried out is very great, and it is a subject that has occupied my time a good deal during the past two years. Experiments have been made on a new road line running through Denham and Atherstone to Manchester and the North; other experiments have been made in South Wales; many have been made in the Midland district; others in the neighbourhood of Newcastle-on-Tyne; some through the Irish Cable; and others on a new line of four copper wires, to which I alluded the other night, between London and the north-west coast. Now the law that determines the value of  $\alpha$  also determines the rate at which a current flows; it determines the number of signals that pass through a line in a given time, and therefore gives us a law by which we can say with absolute accuracy to what distance it is possible to speak. Neglecting the constant, and calling  $S$  the distance to which we can speak, we take the total capacity of a circuit ( $K$ ), which is found by multiplying the length of line by its capacity per unit length, and the total resistance ( $R$ ), which is found by multiplying the length of line by its resistance per unit; and the result is that we get a law determining the distance to which we can speak that is simply expressed by the product  $K \times R$ . From the experiments made, the result comes out that when the speed of current is from  $\cdot 004$  to  $\cdot 003$  of a second, speaking is barely possible; if it is from  $\cdot 003$  to  $\cdot 002$  of a second, it is fair; if it is from  $\cdot 002$  to  $\cdot 001$ , it is good; and if it is below  $\cdot 001$  of a second, the speaking is very good; and, as we have lines constructed of iron, as we have lines that are constructed of copper, and as we have lines that are submarine and underground, we get the following comparative values:—Of iron, 10,000; cables, 12,000; copper, 15,000.

I would call your attention, in the first instance, to the difference between iron and copper. We know, from what we heard in the early part of last session, that the difference between those two is simply due to the self-induction of the iron. Then



see the difference between cables and open copper wire. They are both of the same material; the only difference is that the one is perfectly insulated under the sea, and the other is exposed to the air. The difference between the two is due to the difference in insulation: the leakage that takes place at every insulator supporting an open wire is a means by which the static charge is more rapidly discharged to the earth, and the result is that we get greater speed on copper lines. We can speak to a greater distance on copper lines than we can on iron, and can even speak to a greater distance through an underground or submarine copper conductor than we can through an overground iron wire on existing poles. I think that is one of the most striking facts that has recently been brought out. The difficulties that have hitherto tended to prevent people from carrying wires underground are difficulties very much of the character that I alluded to last time; they are very imaginary. It is possible to work telephones underground with freedom from interruption, with greater accuracy, and to distances just as great as they can be worked by means of iron wires. The experimental evidence is very voluminous; I propose to put it in the form of an appendix, and attach it to the discussion on Professor Silvanus Thompson's paper, and I will not now occupy your time with it. But I might give you an illustration of the formula. Within the last few days we have heard a good deal about some telephonic correspondence that has been carried on between Brussels and Paris. The distance between those two capitals is from 190 to 200 miles, and, for the sake of simplicity, we will call it 200 miles. The resistance of the wire that has been erected between those two places is 4 ohms per mile; that is, it is a No. 11 gauge copper wire which gives a total resistance of 800 ohms. The specific inductive capacity of an overground wire in England varies between  $\cdot 013$  and  $\cdot 0168$  of a microfarad, the latter being the specific capacity of No. 8 iron wire, and the former the capacity of a wire of the same size as that we use—No. 12½ for copper wire.

In England we place upon our poles earth wires, and there is no doubt that these earth wires add considerably to the inductive

Mr. Proce. capacity of our lines—so much so that it is said, and I believe truly, that in America the static capacity of their lines is much less than that of ours. We will take it as it is in England—at .013 per mile—and if we multiply that by 200, we have the total capacity of the line 2.6 mf., which, multiplied by the total resistance of 800 ohms, gives 2,080, which is very far below the copper limit of 15,000.

This law has also been verified in a very curious way by Professor Fleeming Jenkin, who determined from the original formula that it was possible to get through the French Atlantic Cable—2,500 miles long—24 reversals per second; and, by simply following the laws of the square, I find that on the French Atlantic Cable it is possible to get a sufficient number of vibrations to speak through for a distance of 96 miles. When making this calculation I was pleased to find, on referring to one of my early papers, read before the Physical Society in 1878, that through the kindness of Messrs. Latimer Clark, Muirhead, and Company, I was able to repeat this experiment on an artificial cable constructed to duplex the Direct United States Cable—a cable whose resistance and specific capacity was the same as that of the French Atlantic Company—and it there came out that we could speak up to 100 miles; and you will see that the difference between the 96 miles which I got by calculation and the 100 miles got by the experiment in 1878 is not very great.

At the British Association meeting at Montreal, Lord Rayleigh showed by a formula taken from this law that it was only possible to speak through the Atlantic Cable to a distance of 50 miles; but I am afraid that in that calculation Lord Rayleigh took too high a figure to represent the number of sonorous vibrations produced by the voice. It is an extremely difficult thing to arrive at any conclusion as to what is the real number of vibrations per second produced by the human voice. We know that the dominant note of most speakers is not very far from the middle C; but the human voice is reinforced by what are called partials, or overtones, that go up to a great many octaves. The dominant note C gives only 256 vibrations per second, but there are vibrations in our voices that go up to thousands of vibrations



per second. The sibilant sounds, for instance, are probably 4,000 Mr. Preeco. or 5,000, or more, vibrations per second; and Lord Rayleigh, who is the first authority on acoustics, and an authority that I should hesitate very much indeed to dissent from were it not that his conclusion was not quite verified by experimental facts, makes out that the average number of vibrations is about 3,600 per second, and from that he calculates the distance to which one can speak at 50 miles. I think that is too high, and I have taken it at from 1,200 to 1,500. I thought at one time that it probably was about 800; but there is no doubt, from these figures, that we may take the number of vibrations per second at about 1,500. In our actual telegraphic working at the present moment we are transmitting on the copper wire road line to Ireland at the rate of 428 words a minute; whereas the week before the new line was put through the speed on an iron wire line was only actually 130 words a minute, which was the speed obtained simultaneously to Belfast, Dublin, and to Cork: this was not the possible speed, for that was probably 250. On the very day of the change the speed jumped up from 130 to 350 words a minute, the second day it went up to 375, and on the third day it rose to 400, and then reached the top speed of the transmitter—428 words a minute. A speed of 428 words a minute means that we were sending through our cable 193 reversals, or really 386 currents, per second; and we have now our telegraphic apparatus so beautifully constructed that it responds to 386 currents per second. The moral of what I want to urge upon you is that the rate at which we speak, and therefore the distance to which we speak, is solely and simply a question of the circuit, and has nothing whatever to do with the apparatus; for, mark you, the original experiments which I made in 1878 were made with Bell's transmitters, and the last experiments were made with the very best transmitter that I have yet used, a new form devised by Berliner, of Hanover—a form that sends very powerful currents indeed—and yet the rate of speaking has not varied one iota. Therefore I think and hope I have succeeded in convincing you that on this point I am right, and that Professor Silvanus Thompson is wrong.

Next as to the disturbances on the wire. Of course it is all

Mr. Procca. very well, and you will all say so, for me to urge that the distance to which we speak is determined by the construction of the circuit, and that takes you to distances we do not want to reach. We want to talk to distances of 30 or 40 miles or so, where at present telephones are not able to reach in consequence of the disturbances on the line; and Professor Silvanus Thompson will argue, and does argue, that if the receiver is made weak and the transmitter strong, the disturbances due to various causes are toned down. We have disturbances due to secondary currents induced by the primary currents in the neighbouring wires. These vary with the distance separating the wires, with the number of wires on the same poles, and with the length of wire forming the primary and of that forming the secondary wire. But there is this curious fact comes out, that short lines are worse than long ones. The disturbance is due not only to the strength of the primary current, but to the rate at which the primary current rises and falls. It rises and falls with greater rapidity on short lines, and therefore the disturbances are greater on short lines than on long lines; and the notion of getting over this difficulty by making the receiver weak and the transmitter strong is one that has occurred to everybody's mind since the very earliest days of telephony. It was communicated in my first paper read before the British Association, in 1877; it was the basis of the discoveries and work of Edison; it has been the subject of several patents; it is even carried out on the London and North Western Railway, where Mr. Fletcher, the telegraph-engineer of the railway, has weakened the receiver, has strengthened as far as he possibly can the transmitter, and does succeed, for short distances between signal boxes—say for one and a half or two miles—in overcoming the disturbance due to induction from neighbouring wires. But there are no wires carrying Wheatstone instrument currents,—there are no wires on this line carrying Delany currents,—and these currents are so powerful that I defy anybody in this room, with the very best transmitter that ever was produced and the very worst receiver—for these two are really necessary—to speak from the top of the General Post Office down to the bottom if the wire passes through the chasings carrying the instrument

leads to the Central Telegraph Station. It is impossible to speak Mr. Preece. through 100 yards of a heavy street line; it is impossible to speak from the Post Office to our stores at Gloucester Road; and the reason is very simple—that the mutual induction currents from these powerful currents used for Wheatstone transmitters are measurable; they come within the reach of our systems, and careful observations have shown that they are a very large fraction of a milliampère; in fact, currents have been measured giving an indication of about '014, or about  $\frac{1}{1000}$  milliampère. Such currents are probably 100,000 times greater than the currents that are used to work a telephone transmitter; and I quite go with Professor Silvanus Thompson, or anybody else, and say that if it were possible to diminish the sensitiveness of our telephone receivers 100,000 times and still speak, we should do so; but we cannot: we can only diminish the sensitiveness of a telephone receiver to a very small extent, and then we get beyond the limits of the human ear. I think myself that it is folly to carry on experiments in the face of the experience of ten years in every country all over the globe. Wherever telephones have been tried—and where have they not been tried?—everybody knows that the notion that if we are going to get over these disturbances by diminishing the sensitiveness of the telephone receivers and by increasing the strength of the transmitters is absurd. I am sure our friend Professor Silvanus Thompson takes my remarks in the same good part as I make them. In my criticism I do not mean any unkindness to him; I simply wish to criticise and point out what I believe to be not errors of judgment, but errors of fact. Of course where a thing is a matter of opinion it is arguable; but where facts are facts, and where facts are easily expressed by law, then I think we must throw over our opinions and keep to the inexorable logic of facts.

I have just one more critical remark to make. He has given us here a theory of microphonic contacts, and has brought to bear all his poetic skill to explain in very nice language—and he always does write nicely—I know nobody in this Society who knows how to write better than Professor Silvanus Thompson. Take his books. His book on electricity is one of the best books we

Mr. Preest. have ; and his book on dynamo machines is in everybody's hand, and everybody will say that there is no book that has done more to inculcate a knowledge of the theory of a dynamo than that book of Professor Silvanus Thompson. But with regard to that which we may call the heat theory of the microphone, that is a theory that I myself brought before this Society when Mr. Munro's paper was read on "Microphonic Contacts." It is a theory that I propounded after much discussion with Professor Hughes, who agrees with me generally, but I call it the heat theory, while he speaks of it as the arc theory ; and that is the only theory that will explain the reversibility of the microphone. Professor Hughes has shown how it is possible to make a microphone not only transmit speech, but to reproduce it ; and this heat theory—which I say, with all due deference to Professor Silvanus Thompson, is my own—is the only theory that will account for all that. With that exception, I have no other remark to make on this paper. I am perfectly ready to admit that, notwithstanding all the hard words that I have said, this paper is one which does a great deal of credit to the energy of Professor Silvanus Thompson, although I will not say that I think that it will enhance his reputation very much.

Professor  
Hughes.

Professor D. E. HUGHES: Professor Thompson has brought before us in his remarkable paper a series of most interesting experiments, which offer a wide field for discussion. I cannot quite agree with Professor Thompson in his general classification of transmitters and receivers ; nor does he sufficiently indicate those which are merely of scientific interest, but whose results are too feeble for any use, from those which give results of such high value as to be of vast practical importance.

The electric transmission of speech was at first considered impossible ; but since the great advances made in telephony, from 1862 to the year 1878, successive experimenters and innumerable researches have shown that almost all changes in matter caused by vibrations, including the effects caused by heat and light, can be used as transmitters and receivers ; and I congratulate Professor Thompson on having been able to add a new effect, viz., that of the layer between a drop of water and a heated metallic surface,

the water then being in the well-known spheroidal state: this effect, however, must be excessively feeble, and whilst of no practical value it is certainly most interesting from a scientific point of view. Professor  
Hughes.

Professor Thompson has also given us a list of the materials he has experimented with for microphonic contacts. They include a far greater variety than I have been able to try, and I hope that the copper-selenium composition he has mentioned as being superior to carbon for a short time can be so perfected as to render it durable and practical in every respect.

As regards the best arrangement of the microphones, whether in single, parallel, or in series, I believe it entirely depends upon the question of the use for which they are required; and all those who saw my numerous microphonic instruments from October, 1877, to May, 1878, will remember that in some I had them in series, as, for instance, in the glass-tube transmitter, which contained from four to six pieces of hard carbon superposed, thus having six joints or loose contacts in series; and very often I joined the pencil microphones in parallel, as, for instance, at Mr. Preece's lecture in this hall, when the transmitter which was used downstairs to convey the speech and music to the hall had two microphones joined in parallel.

I saw then that there was nothing new, or any new law, that was demonstrated by the joining up of microphones in any conceivable way, for they obeyed in this respect the well-known laws which were known and followed in telegraphy, viz.: If we wish to obtain the greatest possible variation on a line of great resistance, then the microphones should be arranged in series, as we arrange the cells of a telegraph battery, for in this case the internal resistance of the cells is far less than the resistance of the line, and this is equally so with the internal resistance of loose contacts of microphones in series. But if we wish in telegraphy to work on a short line of low resistance, then the resistance of the battery and electro-magnet must be reduced; and so it is with the microphone, for when we employ it on the short thick wire of an induction coil we should employ a battery of large surface or low resistance, and the total resistances of the microphones should be reduced by joining them in parallel.



Professor  
Hughes.

A great deal of attention has been paid to the best arrangement of microphones, but too little attention has been given to the most suitable induction coil. For it is evident that each particular transmitter requires that the battery and the primary wire of the coil should be in proportion to the internal resistance of the transmitter. The secondary wire should also be proportional to the resistance of the line upon which it is used; and from some researches that I have made on this subject it is evident to me that great progress will be made by improved induction coils for telephonic purposes, and Professor Thompson has shown in his paper that he is fully aware of the importance of this question.

I do not quite agree with Professor Thompson in his theoretical explanation of his valve telephone. He considers that the ball or valve does not vibrate itself, but follows or rides upon the vibrations of the air waves. I agree that the ball does not vibrate of itself to any great extent, but I do not believe that it follows or rides upon the vibrations of the air waves. In my mind the true explanation will be found in the experimental fact that the sonorous aerial vibrations impinge on the sides of the tube, putting this into an intense mechanical vibration, and these vibrations are conveyed to the supports of the ball or valve, whose loose contact produces the microphonic effect.

The tube does not become a diaphragm when thus employed, but it is by its solid nature 12 to 14 times a better conductor of sound than air, and consequently the vibrations are conveyed through a tube, or, it may be, a solid rod, with far greater velocity than through the air itself. There is no difference in the mode of propagation of sounds in solids or the air except their greater velocity through solids; and if a solid body, no matter what form or how used, is a legal diaphragm, then the air itself is an English legal diaphragm.

To prove that it is not the ball, but the tube, which vibrates, let us close the tube below the ball, allowing the air to escape by a side aperture in the tube. We shall then find that it works more perfectly than before. Or let us take away the ball and attach several pencil microphones to the exterior of the tube.

We shall then have a very perfect transmitter, fully equal to those disposed on sounding-boards. Professor  
Hughes.

Professor Thompson has shown us his arrangement of 108 microphones attached to a rod in connection with a diaphragm, the microphones being contained in a box the upper portion of which sustains the diaphragm. I do not think this to be a hopeful arrangement. We do not need the to-and-fro motion of the diaphragm. Let us therefore attach these microphones all round the interior of the box, and speak simply in the box, and we shall have all the effects we desire. No doubt we should then have, in addition to our ordinary voice, the hollow tones of the box. Let us therefore fill the box with pure water so as to entirely submerge the microphones. We then find that the hollow tone of the box has in a great measure disappeared, whilst the microphones seem to work with increased power, as every portion of the fluid, being in a state of tremor, conveys to the microphones the vibrations transmitted by the voice to the water and box.

We all know the remarkable power which water has of conveying sound, and to test this, in 1878, I took a microphone to one of our public baths. The room appeared quite silent; but on submerging the microphone in the bath, a perfect uproar of sounds was heard—doors shutting, flowing of water in other rooms, steps, voices, all mixed in a terrific noise.

The remarkable power of the microphone to detect the flowing of water or any leakage in water pipes seems to have received a practical application, for the *Electrician* of January 28th of this year contains the following paragraph:—

“At the recent congress of gas and water engineers, held at Eisenach, in Germany, it was stated that many German water companies had adopted the microphone as a part of the regular equipment of their turncocks, in order to enable them to detect the presence and estimate the importance of any leakage. The microphone is fixed upon a plate which can readily be attached to a stopcock. It is said that the presence of a leakage at the rate of only a few drops a minute can be readily detected by this means.”



Professor  
Hughes.

This paragraph entirely agrees with the experiment that I made with submerged microphones in the bath, and I believe from this and other experiments that there is a field of research open for microphonic transmitters submerged in a fluid contained in receptacles of different forms.

In the portion of Professor Thompson's paper upon telephone receivers there are many interesting and valuable points. He very properly considers that it is not the province of an inventor of telephone instruments to cure the lines of their numerous defects, but to construct instruments which will work satisfactorily through the defects which at present exist.

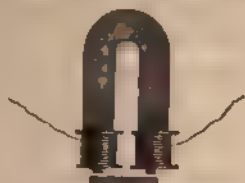
Long or short distance telephony, like telegraphy, does not depend upon the mere distance or resistance of the line, for we have in addition to consider the static and self-induction, both causing a retardation and flattening-out of the extremely rapid waves; and we have also leakage from wire to wire, and electro-magnetic or mutual induction of one wire upon another, causing induced currents, which interfere greatly with those we wish alone to receive. These defects can be greatly diminished by a proper construction of lines; but, taking these defects as they undoubtedly exist at the present time, I do not believe that we shall arrive at better results by the use of less sensitive receiving instruments, or, rather, by instruments having greater inertia in their armatures, and consequently less capacity for rapid currents.

Professor Thompson has shown that a polarised magnet is more sensitive to rapid alterations of currents than a simple soft iron electro-magnet, and this has long been known in telegraphy, for of the innumerable forms of relays that have been tried for rapid telegraphy there are hardly any which have survived which are not more or less polarised by a permanent magnet.

We all know the Siemens form and the Post Office form of polarised relay; but the electro-magnet which is the most highly polarised of all is the one invented by myself and patented in 1855, and still in use throughout Europe in my printing telegraph instrument. In this electro-magnet the soft iron cores of the electro-magnet are simply the prolongation of the poles of a powerful compound permanent magnet.

This form of electro-magnet I found advantageous, first, because it allowed me to have a powerful retaining magnetic force acting on the armature; and, secondly, because I found that

Professor  
Hughes.



an electro-magnet when highly polarised was more sensitive to rapid feeble currents than when not polarised. For this reason the permanent magnet is made to polarise the electro-magnet with far greater power than that needed for the mere holding of the armature; in fact, it is so strong that I have in practice to reduce its retaining effects on the armature by separating it from the poles by a piece of paper. Now this arrangement is well known as the "Hughes magnet," but there is another which is not quite so well known in England.

In 1863, finding it necessary to use a relay which should be equally rapid as the instrument itself, I constructed several relays for the French telegraphic lines in which my polarised magnet was used, acting at a distance upon a soft iron armature similar to other relays, the only difference being the replacing of the old soft iron electro-magnets by the one used in my instrument. This acted extremely well, and it is in use to this day. Mr. Elsasser has made, in Berlin, a particular disposition of relay with the same electro-magnet, which has great advantages for subterranean lines.

Let us now look at the most powerful and successful telephone receivers yet made, as in the box form of the Bell, and in the similar arrangement of the Gower-Bell. The polarised magnet there used is identically the same as mine, and this is a proof that my old form of polarised electro-magnet still remains one of the best for purposes where extremely rapid currents are used.

To Graham Bell and his associates belongs the credit of having produced the best telephone receiver, by a combination

Professor  
Hughes.

for telephonic purposes of an iron diaphragm with a polarised electro-magnet.

The greatest difficulty which is at present encountered on our busy telephonic lines is that due to the mutual induction of one line upon the other, and this would be fatal to sensitive telegraph relays if we had not the power of so adjusting the armature between its contacts that it will not respond to currents less than those intended to be received. In a telephone, however, the diaphragm does not play between any fixed limits, and a very sensitive telephone becomes influenced not only by the desired currents, but also by the extraneous currents.

It is not possible to work on a telegraph or telephone line if the induced currents have an equal or greater power than those we wish to receive. Very fortunately, as a rule, they are far less, and we have then this adjustment of a sensitive telephone, which so far seems the best we can obtain; and that is, to reduce the sensitiveness of the instrument by removing the electro-magnet gradually away from the diaphragm until we can either no longer hear the induced currents, or that their effect is so small as to be hardly appreciable. Unfortunately, in so doing, we also reduce the sensitiveness of the receiver, for its own currents and the sounds received are greatly enfeebled. We can also arrive at the same results by using coarser or less wire on the electro-magnet, using a switch to cut out more or less layers as desired.

We cannot, however, by either of these, or any known method, preserve the loud tones produced by the currents we desire, and reject those which are not desired; nor do I believe that the question can be resolved by constructing telephones of less sensitiveness without any other corresponding advantage.

In all rapid relays we have to make the armature small and light, in order to have as little inertia as possible. Professor Bell, in making a thin iron diaphragm the armature of the electro-magnet, reduced the inertia as far as possible, whilst from its large surface the lines of magnetic force are not confined to any single part. This freedom from inertia allows the diaphragm to respond to an almost infinite rapidity of currents with a perfect sharpness of definition. It is well known that all telegraph relays will work

as receiving telephones by attaching a thread from the armature to a diaphragm of parchment or any suitable material. I believe this was first shown by Breguet, in 1877, who attached an ordinary string telephone to the armature of an ordinary polarised relay. It was also shown by Mr. W. H. Preece about the same time, who, I believe, patented this method; but one of the earliest and most successful attempts in this direction was made the same year at the Newfoundland station of the Atlantic Submarine Cable Co. They attached an ordinary parchment diaphragm by a thread to the coil of Sir W. Thomson's siphon recorder; and as we know the exceeding sensitiveness and rapidity of this coil, we know that the result must have been, as stated, extremely good.

Professor  
Hughes.

Professor Thompson has brought before us some very interesting and novel forms of telephone receivers, and I have no doubt that he has, by comparative experiments, been able to test their respective merits; but I cannot agree with him where he believes that we must follow the lines of a dynamo in order to improve our telephone receivers. The two objects are totally different; though they are both converters of electric energy into mechanical motion, the motion in both is entirely different. The one acquires a continuous rotary motion where the inertia of the armature may be neglected; whilst the other requires a reciprocating motion of great rapidity through an exceedingly small space, requiring also that mechanical inertia must be avoided as far as possible. We have rather to follow the lines of known telegraphic applications, and from these we learn that telegraph relays are far more efficient transformers of feeble currents than any dynamo possible.

There is one point in Professor Thompson's paper to which I thoroughly agree, and that is—If we are to work with more perfection through our present defective lines, we must look for improvement in having more powerful transmitted currents, through a better arrangement of the microphones, and, above all, in the use of a more perfect and suitable induction coil than those in use at present, for we can then adjust or construct our receiving telephones beyond the range of disturbances and receive with perfect clearness, due entirely to the more powerful currents sent by the transmitter.

Professor  
Hughes.

There are many other points of very great interest contained in Professor Thompson's valuable paper, such as the effect of heat on microphonic contacts and his theory of molecular bombardment, but, as I have already taken up more than my share of time, I must leave these to others.

In conclusion, I must express my thanks to Professor Thompson for having brought before our Society a paper so full of interesting experiments, and one so well worthy of discussion.

The  
President.

The PRESIDENT: Having heard two members of the Council, I should like to ask if any members or visitors wish to favour us with their views on this interesting subject. I think it very doubtful that we shall be able to close the discussion this evening.

EUGENE J. MOYNIHAN: I must first apologise for occupying the time of the meeting, inasmuch as telephony is out of the range of my work, and my standpoint with respect to it is almost that of an amateur. But there are several points in Professor Thompson's paper which I think require some explanation.

In the first place, I should like to ask for some details as to the nature of the trials to which Professor Thompson's new instruments have been subjected—particularly as to the lines and lengths of lines on which they have been tried, and with what particular instruments they have been compared. I have read the paper carefully, and can find no information on these points.

Professor Thompson says that no great progress had been made between 1879 and 1885. What progress, if any, has been made since then, if we except the valve and other telephones of Professor Thompson, the merits of which are still under discussion?

Professor Thompson, in his paper, lays down some general principles with regard to long-distance transmitters. The chief difference between these and those laid down by Mr. Shelford Bidwell, in his paper read before this Society in 1883, is that they are not so numerous or precise. Mr. Shelford Bidwell says (*vide Journal*, vol. xii., p. 204):—

- (1.) The constituent elements of a microphone should be numerous.

- (2.) They should be arranged in multiple arc.
- (3.) They should be heavy.
- (4.) The pressure at the points of contact should be light.
- (5.) The resistance of the microphonic system should depend upon the resistance of the rest of the circuit (which should be small), and upon its sensitiveness to change of pressure. In general it should be small.
- (6.) Up to a certain limit, depending upon the number of contacts and their pressures, the current used should be strong.

Mr.  
Moynihan.

Bearing these principles of Mr. Bidwell's in mind, it is somewhat difficult to see what the new points of scientific interest are in Professor Thompson's new and practical instruments. The main point about them seems to be that they are extremely ingenious attempts to circumvent certain galling monopolies.

I should like to call attention to the fact that Edison, in his patent 2,909 (1877), mentions a thermopile arrangement of transmitter, which has the advantage over Professor Thompson's of requiring no flame, the alteration of temperature being produced by the compression of an india-rubber diaphragm in the neighbourhood of the thermopile.

It is a question whether all diaphragms have *appreciable* tones of their own. Has the Blake diaphragm an appreciable tone of its own? In instruments that I have examined the diaphragm is made of fairly thick iron plate, and is not fixed rigidly, but by an india-rubber packing ring. There is also a spring which presses on the diaphragm near its centre with very considerable pressure, for the sake of damping any violent vibrations or tones of its own. Also, with reference to the break in the circuit of the Blake tension regulator, which Professor Thompson finds to be a very serious defect, I am unable to see how it can occur when the instrument is used in the proper manner, for the following reasons:—The carbon button is held on a stiff spring, and adjusted so as to lightly press the platinum head—which is between it and the diaphragm—against the diaphragm; the platinum head is fixed to a very light German silver spring, to which a set is given, causing it to follow the carbon button



fr. Lounthan. for about a quarter of an inch, when it (the carbon button) is drawn back. Now, considering that the inertia of the carbon button is infinitely greater than that of the platinum bead, it follows that the circuit cannot be broken without a very excessive jar. I have never experienced this break, even in an instrument put up in a factory, immediately over the shafting driving some twenty dynamos or so; and it is easy enough to prevent any chance of the occurrence by properly fixing the instrument. When it is roared at, or knocked, or improperly fixed where there is much vibration, there is possibly the liability complained of. But, as far as I can see, precisely the same liability exists in Professor Thompson's instruments.

I should like to know Professor Thompson's authority for stating that the non-necessity of the diaphragm was proclaimed by Edison in 1878. I have not noticed it in any of his patents.

With regard especially to Professor Thompson's valve telephones, I was going to say, in effect, what Professor Hughes has just said as to their action. I should like to ask Professor Thompson whether he has tried to transmit with it with the tube plugged up. I think it will still transmit under these circumstances.

Is not the large inertia of the steel rod supporting half the weight of the carbons in an immense number of multifold-grid telephones extremely objectionable? I hope Professor Thompson will not find it so when his instrument with a thousand carbons or so is built.

Professor Thompson spent some little time in showing (page 6) "the utter fatuity of the dictum that a transmitter cannot transmit unless the contact surfaces are semi-conductors," and that the best transmitter is one which he, somewhat vaguely, does not describe. If this other vague form is the best, why has Professor Thompson, after all, come back to carbon contacts pure and simple?

What does Professor Thompson mean by an induction plug? Is it a shunt? The consequences of every writer of a scientific paper introducing, say, three new technical terms, are enough to *make a humble student's hair stand on end.*



In conclusion, I may perhaps say that I am not pecuniarily interested in any form of telephone, and I should personally be only too pleased to see free trade in telephones. And I may perhaps be permitted to pay my humble tribute to the energy and flow of ideas which have enabled Professor Thompson to essay many hundreds of forms of instrument, with this laudable object, in eight years. I do not know what most people would consider many hundreds, but, taking it at the moderate figure 400, we see that Professor Thompson has for the last eight years essayed a new form of telephone, on the average, once a week.

Mr. A. STROH: I feel somewhat reluctant to speak on the subject, because it is now some years since I have in any way experimented with microphones or telephones; but the section of Professor Thompson's paper devoted to the effects of heat is especially interesting to me, and I may perhaps say a few words on that subject.

Some years ago, when Mr. Shelford Bidwell read his interesting paper on "Microphonic Contacts," during the discussion upon it I described a number of experiments which I then made with an apparatus which I did not show at the time, but which I have brought here to-night. It is simply a very delicate microphone, placed under a microscope, fixed on a little sounding-board, with a watch upon it as a source of sound; one cell of a battery is used to provide a current, and a telephone is used to listen to what takes place at the microphone, so that we have the means of observing by the eye and ear at the same time.

In his paper Professor Silvanus Thompson states that heat improves the action of a microphone, and I have no doubt it does under certain circumstances; but in a small and delicate microphone I do not think any great advantage can be obtained by artificial heat, because I believe that the current in passing creates as much heat as is necessary for the action of the microphone. The arrangement I used consists of a carbon fixed on a board; on it rests another carbon which moves on an axis, and the latter is provided with a spiral spring by means of which pressure can be given and varied at the contact. The axis is vertical, so that there should be no pressure due to the carbon

Mr. Stroh.

itself, and that the pressure should only be caused by the spring. If the spring is slightly tightened—just sufficient to produce what we know as microphonic contact—then on completing the circuit there is generally a burning for a few seconds at the points of contact, during which time the carbons approach towards each other a little, and then the microphone begins to act as such. In the first instance a few points touch only. The current in passing those few points meets with great resistance, and intense heat is the consequence, by which these points are burned and the area of contact is thereby enlarged. This goes on until the resistance is so far reduced that the amount of heat caused thereby is just below that which causes the burning of the points of contact, and this is the condition in which the microphone will transmit sounds. It sometimes happens that the area of contact becomes too large: in that case the current passes without creating the requisite amount of heat, and in that condition the microphone will not work. What takes place, however, between the points of contact when the microphone is in working condition, but in its passive state, as I will call it, is hardly within my province to explain. Professor Thompson has expressed an opinion in his paper on this point, with which I agree, and there is probably some bombardment of particles going on which tends to separate the carbons. The movable carbon then floats, as it were, between this repulsive action and the pressure of the spring.

I should mention, however, an experiment which confirms some of the foregoing statements. I have temporarily replaced the carbons by platinum, and at the points of contact I placed a very small quantity of oil—so small that the whole of it could be seen and observed under the microscope. This arrangement makes a very good microphone, and acts as well as a carbon one, when properly adjusted; but there is a great disturbance at once in the oil when the circuit is completed: particles fly about in all directions, and at the point of contact air bubbles rise and escape, ebullition goes on, and sometimes a thin film of smoke arises, showing the presence of heat. By continued observation of the action through the microscope, and watching the rapidity

with which the floating<sup>2</sup> particles in the oil rotate, a very good Mr. Brooks estimate can be formed of the amount of heat present, compared one time with another; and whenever the microphone was in its best adjustment the amount of heat appeared to be about the same. It also appears that one side of the contact becomes hotter than the other, the particles in the oil flying very quickly from one to the other, and returning slowly. I have tried to establish a connection between this movement of particles and the direction of the electric current, but failed. I have sometimes been able to reverse the effect by reversing the battery, but at other times a reversal of the battery did not make the slightest difference.

The *active* state of the microphone, I think, ought always to be considered quite separately, because the effects which take place during this condition appear to me to be different to those which take place during the *passive* state. If we begin with carbon contacts which are new and have not been used as microphones before, and leave, in the first instance, a gap across the contacts (the battery and telephone connected), and screw up the spring, the very moment that the carbons touch a click is heard in the telephone, and the loose carbon is driven away. If the spring is strong enough to bring it back again, this action is repeated, and goes on in a similar manner as the hammer of an electric bell. If now the spring is tightened up, the number of repulsions become more frequent, until the point is arrived at when the frequency of the repulsions is such as to produce a musical note. The vibration of the movable carbon can still be seen, but as the spring is tightened so rises the pitch of the note, and presently all is quiet. That is the transition from the *active* to the *passive* state. The vibrations which I described just now as the *active* state of the microphone are, I believe, very similar, or the same as the vibrations which are caused when speech or sounds are transmitted through the microphone. I will, however, not attempt to explain their origin, but will simply confine my remarks to the more mechanical effects.

In a microphone which is properly adjusted, and whose area of contact is such as to be suitable for the transmission of currents,

Mr. Stroh. the step from the passive state to the active state is exceedingly small, and the closer the two conditions are brought together the more sensitive is the microphone. If we imagine the adjustment in such a delicate or sensitive condition, we can understand that the slightest mechanical disturbance of the carbons will disturb this equilibrium and throw it momentarily into the active state. I have made some experiments with regard to this condition, but they are difficult to show to the meeting, first, because only one person is able to see or hear what I mean; and, in the second place, because sometimes the arrangement takes a good deal of coaxing and adjusting to get the desired effects. However, we will try. [*Referring to the instrument*] Perhaps Professor Silvanus Thompson will kindly listen at the telephone while I manipulate the adjustment?

Professor S. THOMPSON: I hear clearly.

Mr. A. STROH: Will you please to observe closely the quality or *timbre* of the sound with which the watch ticks? Do you hear a difference in the quality now?

Professor S. THOMPSON: It is quite different.

Mr. A. STROH: By a little "dodging" I can obtain all kinds of quality of sound in the ticking of the watch—sometimes high, sometimes low notes. This effect has interested me very much, for it is clearly not the tick of the watch we hear in the telephone, but *the microphone's own sounds, started by each tick of the watch.*

I have gone a step further, and have thought it probable that the transmission of sound or speech through a microphone is brought about in a similar way as that described in connection with the watch. When speech is being transmitted each single sound wave reaching the contacts must disturb the equilibrium of adjustment, and the microphone's tendency of setting up vibrations of its own must assist the disturbance caused by the sound waves. The two effects, then—the one the result of mechanical movement, the other the result of heat—seem to me to be bound up together.

In other words, it would appear that the mechanical movements due to sound waves have the effect of governing and con-

trolling the frequency, amplitude, and form of the vibrations which the microphone sets up through every disturbance of its equilibrium.

The PRESIDENT: Our time is getting short, but Professor Forbes has a few remarks to make, and if he is unable to finish them before 10 o'clock we must adjourn the discussion; indeed, I intended to adjourn the discussion in any case.

Professor G. FORBES: I understand it is wished that the adjournment may take place now.

The PRESIDENT: Very well.

A ballot took place, at which the following candidates were elected:—

*Associate:*

Edward Ernest Baugh.

*Students:*

Charles Barter.

|

Richard William Hayne.

Charles S. Northcote.

The meeting then adjourned until February 24th.

The One Hundred and Sixty-third Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 24th, 1887—Sir CHARLES T. BRIGHT, M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates were announced and suspended.

Donations to the Library were announced as having been received since the last meeting from the Institution of Civil Engineers; the Committee of the Kew Observatory; J. J. Colman, Esq.; Professor G. Carey Foster, F.R.S., Past-President; R. von Fischer-Treuenfeld, Member; and J. A. Berly, Associate; to all of whom a hearty vote of thanks was accorded.

The PRESIDENT stated that a letter had been received from Lady Bateman-Champain expressing her high appreciation of the tribute paid by the Council and members of the Society to the memory of her husband, and of their sympathy with herself in her affliction.

The PRESIDENT: As a copy of the annual balance-sheet has been sent to each member, I presume that it may, as on former occasions, be taken as read, and I beg to move that it be received and adopted. If any member desires to put any question in reference to it, the Honorary Treasurer or the Secretary will, I am sure, be happy to afford any further information required.

Mr. J. N. SHOOLBRED seconded the motion.

Mr. E. GRAVES: I should like to make a remark in respect of one item in the balance-sheet. It will be seen that among the items of "extraordinary expenditure" there is a sum of £50, representing the Society's contribution towards the expenses of the President's Conversazione. Through the liberality of our Past-President, Professor D. E. Hughes, I am glad to announce



that he has presented that sum as a contribution to the Society's Telegraph Jubilee Fund, about which you will shortly receive circulars; and I am sure the members will duly appreciate his kindness.

The announcement was received with applause.

No question in reference to the balance-sheet having been asked, the PRESIDENT put the motion, which was carried *nem. con.*

The adjourned discussion on Professor Silvanus Thompson's paper on "Telephonic Investigations" was then resumed.

Professor GEORGE FORBES described and exhibited in action a new telephone transmitter which had been shown to the Royal Society the same day. It consists of a fine platinum wire stretched in a slit in the base of a mouth-piece. This wire is heated by the passage of an electric current. The vibrations of the air cool the wire, diminish its resistance, and vary the strength of current, thus causing it to act as a transmitter, articulate speech being fairly well reproduced.

Professor  
Forbes.

He then asked Professor Thompson to eliminate from his paper the paragraph in which he accused the scientific world of ignorance about the action of polarised relays and telephone receivers. Professor Thompson saddles us with the belief that because the variation of magnetism due to a small change of current does not increase with the initial magnetisation, therefore initial magnetisation ought not to increase the sensitiveness of a telephone receiver or a relay. This may be Professor Thompson's view, but he has no right to charge us with the same. Of course the sensitiveness of the instrument is proportional to the attraction exerted, which itself is proportional to the square of the magnetisation. If  $M$  is the initial magnetisation, and  $m$  the variation due to the current, the attraction when the current is flowing and when not flowing is  $M^2 + 2 M m + m^2$  and  $M^2$  respectively, and the difference is  $2 M m + m^2$ , which represents the sensitiveness of the instrument. This is enormously increased by making  $M$ , the initial magnetisation, large, even though  $m$  were somewhat smaller for high degrees of magnetisation.



Major-Gen.  
Webber.

Major-General WEBBER, C.B.: We have not heard much in the course of Professor S. Thompson's paper of the good qualities of his membrane receiver; but I hope in his reply he will tell us a little more, not only as regards its ingenious construction, but also the experience that has been acquired in its use. I have from time to time found his membrane receiver in use on small telephone lines in various parts of the country, and on no occasion have I found any of them out of order. Considering that the membrane under normal circumstances must be very susceptible to damp, it shows that his construction and mode of covering it must be good to have attained success with telephones that have been installed for considerable periods of time in damp situations. In connection with the sentence where Professor Thompson says that the clue to long-distance telephony is being followed up in the way he has described, we all must have regretted that Mr. Preece brought our minds immediately, with reference to that part of the subject, to the contemplation of the comparatively large currents which are used with intermitting telegraph machines, and did not sufficiently impress upon us the difference of the nature of the work performed under the two conditions, viz., by currents used in that way and telephonic currents. We all know that with such machines as the Meyer, Schæffer, Baudot, and others, in which intermittent currents are used which are undoubtedly caused by distinct makes and breaks, we are dealing with a current of '012 of an ampère, or something of that kind, and that the capacity of a line for those currents is soon reached; and the state of a line when the intermissions are too rapid becomes what used in America long ago to be called a "blocked current." These systems are rather means of mechanically analysing those currents, as compared with the reception of telephonic currents; but there is one system which I think more nearly approaches the old description of the telephonic current—viz., the undulatory—in its manner of reception of the electricity, and that is the harmonic system of Elisba Gray. There you have a totally different kind of analysis, and one far more resembling, far more capable of being used as a practical illustration of the

action or the variation of intensity of telephonic currents, and far more likely to elucidate that question of the capacity of a line for long-distance telephony, than any of the instruments to which Mr. Preece referred.

Major Genl.  
Webber.

I am sorry to have again to refer to what Mr. Preece said on the first evening of our discussion, and I am sure that when he saw the shorthand writer's notes of what he had said and the words he had used, no one regretted more than he did the apparent aspersion which he cast upon Professor Silvanus Thompson's motive for bringing his paper before us. It is a subject which, of course, one would rather avoid; but, considering that this is the first year since the introduction of the telephone that our Society has taken up this question seriously, I think we must not forget that that very thing which Mr. Preece deprecates has become a fact, and that is, that the use of the telephone is a monopoly. Now none of us have any wish other than to respect the rights of inventors, and to give them the respect and reward that they are entitled to, and not only themselves, but those to whom they have transferred those rights; but when we find that the combination of a tension regulator of carbon and of a tympan or diaphragm is to be interpreted in the way in which it apparently now is, when almost everything is a carbon tension regulator, and the human body even is classified as a tympan, I think we would all of us say that it has gone too far. Now no one who recollects the acclaim with which Professor Graham Bell was received in this room many years ago can say that we in England did not wish that he should receive all the honour to which he is entitled, and be well rewarded. But it is now nearly ten years since something occurred in Paris—of which I am probably the only witness now alive—which I think points out very clearly what was the mind of Europe at that time in connection with that great invention, and also shows that, although people were ready to give Professor Bell all the credit that was his due, they also regarded others as having equal credit as discoverers in the same direction. I was British Juror for telegraphs and electrical apparatus for the International Exhibition at Paris in 1878, and, under the presidency of M. Becquerel—a very distinguished Frenchman, and well known in

Major-Gen. Webber. } electrical annals—the jury considered Edison, Bell, and Elisha Gray as probable candidates for the Grand Prix. As vice-president of the jury it was referred to me to make a report as to which of those three I thought was entitled to that, the highest distinction that was then given in France. Count du Moncel, a very well-known expert, was asked to assist me, and we worked together for one afternoon examining all the documentary evidence (including the caveats of 1876 of both Bell and Gray) which was then to be found in Paris. There was no question as to Edison's claim to honour; but when we came to examine the claims of Bell and Gray to be the discoverer of the articulating telephone, we arrived at the following conclusion:—That, while Gray was the original searcher after an articulating telephone, Bell was the one who discovered it; *i.e.*, that Elisha Gray, while searching to produce an articulating telephone, found a harmonic telegraph, and that Bell, while searching for a harmonic telegraph (which was what he wanted), found the articulating telephone. Under those circumstances I reported, with the full concurrence of M. du Moncel, and it was unanimously accepted by the jury, that those two gentlemen were equally entitled to the Grand Prix; and if our judgment was a right one I can only say that, while Bell has received his desert both in honour and reward, Elisha Gray has fallen far short of what he ought to have received for his exertions over many previous years, and that the monopoly which has been conferred by our courts of law on the owners of the Bell and Edison instruments embraces a much wider field than equity justifies.

Professor Ayrton. } Professor W. E. AYRTON: The question as to whether initial magnetism increases the sensibility of magnets was fully developed in the *Electrician* of February 19th by Mr. Oliver Heaviside, who, in a most interesting contribution, mentioned the expression which was, I think, fairly well known among electricians some time ago. It is not, as Professor Forbes has pointed out, simply a question of the addition to the amount of induction that is produced by a given addition to the magneto-motive force that settles whether it is well to have a large initial magnetism or not, but the product of this extra induction into the magnetising

force that determines the change of force exerted on an attracted body, and which is what we desire to make large. Professor  
Ayrton.

Professor S. Thompson has given a very poetic description of the action of the microphone: it is an almost Tyndalian account of the bombardment of the molecules; and, do you know, I think that Professor S. Thompson not only shares my admiration of the beauty of natural phenomena, but is carried away by it to use an elaborate description for a very simple effect? No doubt the action of the arc lamp can be poetically described as the bombardment of the particles going from the positive carbon to the negative carbon, but for ordinary purposes we call it an arc lamp; so for ordinary puposes I am inclined to think that we must call the action of the microphone the action of a very great number of small arcs. My reason for thinking so is the result of experiments described in a paper by my colleague and myself on the resistance of the electric arc, by which I mean simply the ratio of the potential difference of the carbons to current—for there is no other meaning—varies with the current in such a way that the potential difference between two carbon points at a fixed distance apart is nearly constant for very different currents. And it was shown—if I remember correctly, by Mr. Shelford Bidwell—that the resistance of a microphone varies in much the same way; and we pointed out at the time that this similarity in the variation of resistance with the current being of the same kind in the two cases, there was a good reason for thinking that the circuit in a microphone and the circuit in the arc were much of the same nature. And I still think that there is a very good reason for thinking that since the variation of resistance of a microphone and the variation of the resistance of an arc follow almost identically the same laws it is probable they are the same in action. The question as to the reason why heat improves the action of the microphone is interesting, and I think that if it were possible to heat the space between the carbons an arc might be started without putting the carbons together at all. Heat diminishes the resistance of the air, and I believe that if the action of the microphone is improved by heat it is because heat diminishes the resistance. Why heating one part of the

*Dr. Thompson  
Lyrna.* microphone improves the action, I think, is for the very same reason that in the ordinary arc, as we know, the positive carbon is very much hotter than the negative carbon, for this is a state which facilitates the discharge of the shower of carbon particles.

Going to the question of magnetism, Professor Thompson mentions that for his induction coil he tried an iron ring—that is, he used a closed magnetic circuit—but was compelled to give it up on the score of expense. There seems to be a certain amount of vagueness as to whether a closed magnetic circuit should or should not be used in an induction coil, and I think the reason is that sometimes inquirers bring high mathematics to bear on the subject, and do not apply the simple reasoning of common sense. I may remind you that Lord Rayleigh, on this very point, made a curious mistake, which he has since corrected, in considering whether a closed magnetic circuit should or should not be used in a secondary generator. He mentioned that his opinion was that a closed magnetic circuit was very bad for a secondary generator; but of course all practical men know that that is not so, but that a closed circuit is very good for a secondary generator. The real fact is this: the late Comte du Moncel told me, many years ago, that he tried it for an ordinary induction coil, and found that it was not good. He used an ordinary primary and secondary coil, and by means of a contact-maker got an induced current; he thought he would improve the action very much by making the core like that of a Gramme ring, but he found, on trying, that he got a very bad induction coil. We know that in a closed iron ring it is very difficult to remove the lines of force by merely stopping the current, and that was why Du Moncel failed to get good results, but he did not know why at the time. If, however, the current be reversed, then a closed ring is better than a piece of straight iron such as is ordinarily used in an induction coil. Therefore, in 1872, I added a reversing contact-maker to an induction coil I had in Bombay, and then a closed magnetic circuit was a great improvement. If I understood Professor S. Thompson's plan properly, he never reversed the primary current of his telephone induction coil, but only altered its strength; and under those circumstances



it seems to me that it would be quite a mistake to use a closed magnetic circuit, and so far from obtaining a better induction coil by using a closed circuit a worse effect would be obtained. But in an induction coil in which the primary current is reversed a closed magnetic circuit would be an advantage.

Professor  
Ayrton.

Last time Mr. Preece entered into that beautiful solution that Sir W. Thomson gave some years ago by employing what he has himself called a "mathematical poem," viz., Fourier's "*De la Chaleur*," Mr. Preece showed how Sir W. Thomson determined the speed of signalling in a submarine cable; and, if I am not mistaken, Mr. Preece implied that the same reasoning was sufficient for a telephone line. I venture to differ from him altogether on that point. No doubt we shall have to take into account the retardation arising from the fact that the wire acts like a cable; but I believe in telephone work, with overhead wires, the question of mutual induction and self-induction is infinitely more important than the question of electrostatic induction. The laws for mutual induction are not a bit like the laws for electrostatic induction, and therefore they do not apply. Do not let it be supposed for a moment that I am even suggesting that Sir William Thomson did not think of this, because in his original letter to Professor Stokes he said distinctly that if the cable be coiled in a tank then the retardation due to self and mutual induction must be taken into account; he therefore intimated that there was something else to be considered. But a straight submarine cable is a conductor with great electrostatic capacity and very small self-induction, whereas an overhead iron wire is a conductor with very small electrostatic capacity and very large self-induction. Indeed, Mr. Preece has told us that the electrostatic capacity of an overhead telegraph line per mile was about 0.013 microfarad. We know that the capacity of a submarine cable is about 0.3 or 0.4 microfarad per mile; therefore the capacity of a submarine cable is about 30 times the capacity of an overhead telegraph line. The resistance of iron is only about seven times the resistance of copper, and we know that the speed of working, as far as electrostatic retardation is concerned, depends upon the resistance per mile and the capacity per mile;

Professor  
Ayrton.

consequently, as far as electrostatic retardation is concerned—which was what Sir William Thomson considered in his famous solution of the problem on the speed of signalling—speaking by telephone will be more readily done through a long iron wire than through a submarine cable; but, as Mr. Preece has said, we cannot telephone more quickly through an iron wire than through a submarine cable. Now the reason of this is because the self-induction of the iron wire is much greater than that of the copper cable conductor. That shows that it is self-induction, and not simply electrostatic retardation, that must be taken into account.

As to self-induction, I have already hinted that the laws are quite different. I have not had time to work out the complete problem to obtain the laws of speed of signalling through a wire, taking into account electrostatic capacity, resistance, and self-induction; but we know quite well from our experience with ordinary alternate-current dynamos what is the sort of law, as far as the self-induction of the circuit affects the problem. We know, for example, one great difference that exists between the retardation arising from electrostatic capacity and the retardation arising from self-induction. In a submarine cable electricity is poured in at one end, but hardly any at first received comes out at the other end: it is somewhat similar to a long narrow tube into one end of which water is passed, and the sides of which yield much under pressure, so it is only after a large quantity has been put into the tube that any begins to come out at the other end. Now self-induction acts quite differently; it does not make the current at the far end of the line different from that at the sending end. The current is the same throughout the line; all that self-induction does is to make the sending current weaker by exactly the same amount as it makes the receiving current weaker, and it retards them both equally on phase—i.e., it makes the signals a little behindhand—but while it delays the effect it does not delay the effect at one end of the line more than that at the other. Self-induction, then, delays the signal and it diminishes the strength of the signal; and what I want to impress upon you is that that produces exactly the same effect at both ends of the line, which is not at all the effect of capacity. Therefore I should like



to modify the calculations arrived at last meeting by Mr. Preece which only applied to electrostatic induction, by adding the necessary correcting factors depending on mutual induction, and I venture to think that the results would be very different from those he arrived at. But, be that as it may—whether it be true or not that self-induction is the important factor in telephoning, and not electrostatic capacity (I mean for overhead telephone lines)—I do not think that Mr. Preece's conclusion was right. His conclusion was that because Sir William Thomson had shown that the speed of signalling depended on the square of the length, &c., therefore it was unnecessary and useless troubling in the slightest about the receiving instruments. But Sir William Thomson did not regard his own result in that way. The very first thing he did trouble himself about was the sending and receiving instruments, "because," he said, very naturally, "if I cannot alter the condition of the line, I can alter the condition of its ends;" and you know that the reflecting galvanometer came out of it, as also did the siphon recorder, so did the curb key, so did the late Cromwell Varley's excellent condenser method of signalling still employed; and I have not the slightest hesitation in hoping and believing that in some few years we shall have special instruments for sending and receiving by telephone. What I object to is the conclusion that, because there are certain definite laws well known to mathematicians about the speed of signalling through long wires, we should not trouble ourselves about the receiving instrument.

After the attacks that have been made upon the reader of the paper, I should like to say just one word about the question of patents. I happen to be in a very peculiar position with reference to telephones in that I do not own shares in any company whatever (I only wish I did). I have no interest whatever, directly or indirectly, in any telephone company or telephone, and I am, perhaps, therefore in a better position to consider the matter more from a scientific point of view than if I had an intentional bias in some direction. Now what did Bell do for us in 1876? I think he made one of the greatest discoveries of modern times. He showed that an instrument made not unlike

Professor  
Ayrtoun.

a sounder could be made to speak to another instrument not unlike a sounder; and I venture to think that, whatever may have been said, nobody before Bell anticipated that in such a simple way speech could be transmitted. But the question is, Do we use that method of transmitting speech? We do not. Between two rooms at our college I am able to use two Bell telephones, one as a transmitter and the other as a receiver, but in ordinary commercial work two simple Bell telephones will not answer; certainly, if they were used in London, telephoning would be even worse than it is at present, if possible. Of course it could not be much worse than at present, because the lines are *even still* broken down, but I mean would be worse even if the lines were up. What, then, do we use? Some form of carbon transmitter: whether it be called a Hughes, Blake, Johnson, or anything else, it is some form of microphone. Now the microphone is not claimed by Bell. I am not going into the question whether Edison's carbon button is a microphone—I venture to think it is not—but that is not the transmitter we use; and, that being so, I do not see that it is an infringement of Bell's patent or an attempt to evade Bell's rights by endeavouring to do something or other that Bell never did himself.

The  
President.

The PRESIDENT: We cannot adjourn this discussion again, and I would therefore ask any other members who are desirous of speaking to make their remarks as concisely as possible, in order to give Professor Thompson an opportunity of replying this evening to the different arguments and observations which have been made. I am sure that we could not expect him to do so much under three quarters of an hour.

Mr.  
Yatman

Mr. H. G. YATMAN: We have heard frequently here, much about the action of microphones and telephones, and I venture to ask whether we may not now say that microphones and some telephones speak to us by sonorous electrical discharges. Do not suppose that I am rushing in where wise men have said nothing, for in support of this contention I will bring names that all here respect. All must regret that Professor Hughes refused to consider the molecular bombardments between microphonic contacts, which Professor Silvanus Thompson figured

to the imagination, and asked, "Will any one undertake to say <sup>Mr. Yatman.</sup> that no discharge there takes place?" Mr. Stroh advances the matter when by aid of the microscope he shows to the material eye, with platinum contacts and a fragment of a drop of oil, these bombardments, and further adds that carbon contacts restrained by a spring in these movements give a note—a singing. Again, he expresses a belief that the microphone has sounds of its own, governed and controlled by sound waves when spoken to. In support of this let us go further, and in a direct battery current add to a microphone a "grid" microphone as telephone. Placed in the ear, one hears words spoken in the microphone. Have we not here an electrical discharge? I must not detain you with many examples, but take you to the experiment of the late Mr. Dunand. Mr. Giltay, writing of this in a paper on the polarisation of telephonic receivers, refers to a microphone with a battery in the primary wire of an induction coil, a condenser with a battery of 15 cells in the secondary wire, and shows in some detail how this condenser, charging and discharging itself, produces perfectly what is spoken in the microphone. Nay, he even says, "Thanks to the passive character of the leaves of tinfoil, you hear the sound reproduced more naturally than in the Bell telephone." Again, Mr. Giltay puts two condensers in a battery circuit with a selenium resistance and makes them speak to each other—a good example of charge and discharge, if the last experiment can be called such. To be brief, I will only further consider Mr. Giltay's famous experiment, in which he uses the arrangement of Mr. Dunand, only in place of the condenser putting two living beings, each of whom takes a terminal of the secondary wire in one hand, and, placing the free hand on an ear of a third person, makes him to hear all that is spoken to the microphone. What happens in this case? Without anything to move, with no apparatus beyond the hands, they give what is spoken to the microphone in what I invite you to call sonorous electrical discharges. That they are sonorous there is ample evidence; that they are electrical discharges I ask you to say with Mr. Giltay and Mr. Hospitalier.

Professor J. A. FLEMING: I would just like to make a very <sup>Professor Fleming.</sup>

Professor  
Fleming.

few remarks with respect to that section of Professor Thompson's paper which deals with what he calls the "transformer," or, to use a more familiar word, the induction coil. The important quantity which has to be considered in an ordinary aerial line in which electrical pulsations are being set up is that to which Lord Rayleigh has given the name of the "throttling," and which is measured by the square root of the sum of the squares of the ohmic resistance and product of frequency and self-induction, or, symbolically,  $\sqrt{R^2 + p^2 L^2}$ . In a telephone line the self-induction of the secondary circuit is due partly to the line and partly to the secondary coil of the transformer, and it is obviously desirable to so proportion the circuits of the transformer that the coefficient of mutual induction is as large as possible and the coefficient of self-induction as small as possible. Unfortunately it is only too easy to do the opposite, but I cannot help thinking that a very careful investigation would reveal possibilities of greatly improving the design of transformer now used, with benefit to the general result. To pay attention wholly to the line, and not at all to the transformer, is not entirely to have dealt with the difficulties of telephonic transmission.

Professor  
Thompson.

Professor SILVANUS P. THOMPSON, in reply, said: Mr. President and gentlemen,—It is the custom in another place, when a member rises for the first time, to deal tenderly with his maiden effort; and though I can hardly say that the occasion of the reading of my paper is the first time that I have risen to speak in this room, certainly my maiden effort at writing papers for this Society has received a large amount of kind attention from my friends. I certainly cannot complain on that score; and when I rise to answer the criticisms that have been made, the difficulty is to find something that has not been answered, because it seems to me that nearly all the speakers who have taken part in the discussion have either answered one another or have touched upon the matter in such a way as to leave me scant room to reply. However, I propose to say something in reply to every speaker, and I will deal with them in the reverse order to that in which they have spoken—the last first.

Professor Fleming has certainly not emphasised the importance of the induction coil, or transformer, more than I myself would wish to do. I did not in my paper enter into all that I have done in the way of constructing induction coils. For example, I had one here, but it was left in the bag underneath the table—an induction coil or transformer of the form of a short cylinder of large diameter, the core consisting of a bundle of iron wires about 5 centimètres in length. The secondary was wound in two halves, with the primary coil between. That form worked extremely well, but it was rather awkward to build. I considered also how to make the mutual induction a maximum without increasing the self-induction. Suppose you have a primary coil with self-induction  $L$ , and the self-induction of the secondary is  $l$ , then the square root of the product  $\sqrt{Ll}$  would be equal to the mutual induction between those two, provided the primary and secondary circuits could be brought to absolute coincidence with one another. This is, of course, physically impossible in many cases, but may be done by putting one conductor inside the other, a wire going inside a tube. But as a matter of fact the coefficients of mutual induction are always less than the geometric mean of the coefficients of self-induction. If you take two coils and put them miles apart they have self-induction, but no mutual induction. If they are made to approach to within a short distance there is mutual induction, and the nearer they approach the greater is the amount of mutual induction. Of course their apparent self-induction may be altered by the presence of the mutual induction, as everyone knows who has worked with alternate currents for distribution purposes. My object was to obtain a shape of coil which would, for given values of self-induction, have the maximum of mutual induction. Now it occurred to me that in the usual elongated form of induction coil the very form involves a greater amount of self-induction in the secondary circuit than is necessary in obtaining the requisite mutual induction. From the purely self-induction point of view the elongated cylinder with an iron wire core is about the worst form of coil you can have. Take simply, say, a telephone receiver, and suppose you are going to put into that a certain amount of fine wire, and you have a

Professor  
Thompson.



~~Professor Fleming~~ magnet and an iron plate: how will you wind it? Will you wind it all along the iron in a narrow cylinder? or will you put it at one end, and, if so, what form will you give it? Of course you want to have the greatest effect at the magnetic gap between the pole and the plate, which is the point of greatest sensitiveness in the magnetic circuit; but you will get, most certainly, far less self-induction, and far less "throttlng," if you wind your wire, as is always done, in a small coil at one end, than if you wind the same amount of wire along the magnet. It was described long ago in Bell's own researches—how he gradually shortened and shortened the winding till nothing was left but a coil on one end. Why? Because if you put the coils all along you may get a larger magnetising effect for steady currents, but you will also get a reduction in the magnetising effect of rapidly reversed currents, because the additional turns of wire introduce disproportionate self-induction, and are actually detrimental. I should add, in reply to Professor Fleming's further question, that I think it beyond doubt that every induction coil tends to transmit electric impulses of one periodicity better than another. It depends on the construction of the coil.

Professor Ayrton pointed out—what is perfectly true—that a closed magnetic circuit is of no value *per se* in a transformer, unless you reverse the current. You must not simply break it; you must reverse it. That is exactly what I do. I employ two primaries, as shown in the arrangement of my differential circuits shown in Figure 8, with a transmitter in one circuit and a small resistance—which I call a balancing resistance—in the other circuit. The battery current divides between the two. If the resistances of the two parts were equal the current would flow equally each way and there would be no magnetism at all. But I find the best effects to be given when the balancing resistance is a little less than the average working resistance of the transmitter. I have tried to measure this resistance in various ways, and I have come to the conclusion that my core was actually having its magnetism rapidly reversed while the transmitter was at work. Among the coils I have used was one made in annular form, the core being of fine wire. The arrangement was exactly like

Faraday's original ring, the first transformer ever made, and which may be seen, represented in marble, held in the hand of the statue of Faraday where it stands in the Royal Institution. That ring, having a closed magnetic circuit with a primary and a secondary coil upon it, I take to be the true type of all transformers. I got good results from that arrangement, but it was at least five times more costly to make than the ordinary pattern that can be wound in the lathe, and I gave it up on that account. I then tried another method. I had not then heard of Ziperowski and Deri's method of making transformers, and therefore my work was independent of theirs. I took a hank of fine wire and a hank of coarse wire, as primary and secondary. I divided the coarse wire into two parts and put the secondary coil between. I then pushed a quantity of fine iron wire through the aperture, and bent it down all round so as to pretty nearly form a closed magnetic circuit, through the inside of the coils. But that did not give much better results, considering the amount of wire employed in the two coils, than the same amount of wire gave if built in the ordinary form; it did not give so much better results as to make it worth while to spend upon it the additional labour and time, and therefore my labours in that direction came to an end. I think, however, the result of this discussion will be to make me begin again and try if I cannot get some form of transformer which will be no more costly and have the additional virtue of less proportional self-induction.

As to the sonorous discharges which Mr. Yatman referred to, I would only point out that among the numerous kinds of receivers that are classified in my table there comes in this very one, under the electrostatic head, as the "air sound." When reading my paper I referred to the fact that you can hear a sound outside, such, for example, as a large brass ball mounted on a glass stem—a familiar old-fashioned piece of electrostatic apparatus. If you connect such a ball to one terminal of the secondary circuit of an induction coil, the other end being put to earth, you can hear a sound outside it. When so connected that ball is being charged and discharged from the wire, and there is a varying electrical pressure upon the air outside it. That is a perfectly



Professor  
Thompson.

well-known fact, and we know how it comes about. We know that when a soap bubble is electrified it alters its size, in consequence of the change in the air-pressure due to the charge upon it. It is a most beautiful experiment. The air outside the brass globe undergoes this varying change of pressure of the layer close to its surface, and it sets the air around it vibrating, and you can hear it. I think that that must have been the case in Mr. Giltay's experiments referred to by Mr. Yatman. When he took the electrified hand and placed it near another person's ear he heard the hand talking. I think in such a case the person's hand simply forms part of a condenser.

Referring to Professor Ayrton's remarks: I quite expected to have the microphonic effect described in the course of this discussion as being a sort of arc, but I am not quite sure whether that adds to our knowledge; we really know so little about the structure of the voltaic arc, and it is so hard to examine because it is so hot. I would rather say that the arc is a sort of microphonic contact, so far as there is any similarity between the two. I am quite prepared to believe that there are molecular bombardments going on in both, but I think that they differ both in degree and in kind. I would ask you to regard the curious state of things which is called the spheroidal state; as also I would draw your attention to the researches of Barrett, Stoney, and others, in which they prove that a drop of liquid laid upon a hot metal plate is supported above the plate, separated from touching it by a layer of vapour, the vapour exercising in this curious state an upward pressure, due to the fact that the lower surface is being subjected to the bombardment of the particles below. The particles are going up faster than they go down, simply because the bottom surface is hotter than the top surface. I could hardly wish for a better explanation than that afforded by Mr. Stroh's microscope experiments where he saw the particles flying quicker in one direction than in the other: they could not have done so otherwise. There is really a bombardment of flying particles going on—loose and free particles in the case of the microphonic layer when there is no actual touching of the solid surfaces. I do not say that at all stages in the microphonic

motion it is so. I imagine that in some stages the upper part comes down on the lower and rests upon it at certain points—three at least in number, possibly more—because some parts possess more or less elasticity of form; but still there will be this continual heaving up and down, and the contact surface will be beaten back. In this battering back, with the discharge of electricity that occurs in that action, I think you have the outlines of a future theory of the microphone. I do not think you have that in the arc, although you have undoubtedly bombardment; but it is not simply particles flying across from the hotter of the two poles to the other and back again. I cannot think that the mean length of the free path extends from pole to pole in the arc; because I think we should get polarised light from that, and should have a very different state of things if we had a real Crookes's layer between the two surfaces. But my impression is that you have this mass of glowing vapour, which does not give much light (because the ends of the carbons give out the light), in which there is an internal commotion similar to that of an ordinary heated gas, and not a state of "heat polarisation" as in the Crookes's layers and in the spheroidal state.

Professor Ayrton suggested an experiment upon an arc, and as I happen to have made that experiment (so far back as 1878) I think I can answer the question. It was this: Suppose you put a couple of carbon pencils into the circuit of a battery or dynamo to give you the requisite electro-motive force, but instead of bringing them into contact simply leave them apart from one another, and then try to start the arc by putting a flame in the space: will the arc start? No; it will not. I have tried it with gas flame and with oxyhydrogen, but could not do it—probably because the temperature was not high enough. But it is the easiest thing in the world to start an arc between two poles which do not touch if only once you will send a very minute spark through it. Take a common electric gas-lighter, or a small induction coil, or anything that will give the very smallest spark that will stretch across. The arc seems to require a path finding for it.

Professor AYRTON: Did you rarefy the air?

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Professor THOMPSON: No.

Professor AYRTON: My proposal was to rarefy the air as well as to heat it, so as to get a break in air of very low resistance. I am much obliged to you for the answer to the question.

Professor THOMPSON: Returning for a moment to the transformer: Professor Ayrton referred to the experiments of Count du Moncel. May I point out that there exists another comparatively early contribution to this question of whether transformers work better in closed circuit or not. There is an experiment in an old volume of the *American Journal of Science*, of some ten or fifteen years ago, related by Professor Trowbridge, who took two ordinary induction coils so arranged that the end of one core was a north pole when the other was a south, there being also two pieces of "tin-plate" (i.e., thin tinned iron sheet) opposite their ends to complete the magnetic circuit across from one to the other. He found that this made an extremely good induction coil. If it had only occurred to him to put his ear to the back of these tin plates, he would have heard a good deal going on at them; they would have been magnificent receivers for telephonic purposes.

I was asked a question by General Webber about the membrane form of receiver. Well, I do not know how far I can reply to that. It is quite true that a good many of that form of receiver are to be found up and down the country, and they are practically all of the same pattern. A membrane is stretched over a short metal cone or tube, somewhat like the cover on a jam-pot, with a little piece of iron in the middle, secured to the middle of the membrane. There is no originality in that arrangement. I only use it because it appears to me to be the least open to cavil in certain directions; and it is an uncommonly good form of receiver. Certainly I find very little trouble indeed from any effects of damp. It is very easy to prepare a membrane so that damp does not make it very bad or flabby, and it is very easy to do even with a flabby membrane what you want. Part of the secret of success lies in the way of arranging the attracting pole behind it. In the receivers I use the little circular piece of iron is about the size of the thumb nail, and the attracting poles

act, one at its centre, the other at its circumference, so that it is drawn to and fro as a whole. Experience has shown it to be a good form. Professor  
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I have found it expedient to eliminate eddy-currents by the same sort of device as that by which eddies are eliminated in the armature of a dynamo, namely, by lamination. I cut a slit in the round iron, ending in a hole in the middle, so that any eddy-currents circulating in the disc should be cut off by the slit. Although that is a small improvement, it has, I think, certainly its value.

Then I come to the remarks made by Professor Forbes. The explanation respecting the stress on the receiving diaphragm, as calculated by the formula which he wrote upon the board, was practically a repetition of that given by Mr. Oliver Heaviside in last week's *Electrician*, and it is one answer to my statement that there was more to be taken into account in that matter than was taken into account by M. Giltay, who specially studied this question why an initial attraction is necessary. He found it necessary in his statical receivers or condensers, as well as in the ordinary magneto-receiver, and he deliberately states in his memoir on the subject that the *only* reason why an initial pull is required was to obviate double vibrations. Now I doubted it, and I said there was more than that. It did not occur to me, I am quite free to confess, that that very simple explanation about the difference in attraction was one portion of the explanation of it; but it is very simple, and now that one has been shown it, it is very obvious. But that is not yet the whole of the facts; there is something more than that still, and that is what I wanted to have some light thrown upon to me.

When I read my paper here I brought, among other things, a little piece of apparatus—a sort of induction coil—for trying this question. It is not a question altogether of attraction: it is not whether the variation of the attraction is greatest when the magnetism is great; there is a question of variation of magnetism that comes in. In this particular case I took a small piece of iron which is exactly such as you would use for the core of the bobbin of a telephonic receiver, about an inch long

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and about one-fifth of an inch in thickness. Upon it were wound three coils—a primary of fine wire, a secondary also of fine wire, and a third coil of thicker wire over the outside, into which was led a current from a few accumulators through a variable resistance. The secondary was connected to a receiving telephone, which therefore would give out sounds when the magnetism of the core was varied. I wanted to find whether small rapid variations of current in the primary coil would produce the greatest variations of magnetism (and the loudest sounds in the telephone) when the core was not magnetised at all to begin with, or when it was a little magnetised, or when it was magnetised very strongly. To furnish small rapidly fluctuating currents in the fine-wire primary it was connected to the primary of a small ordinary induction coil the primary of which was in circuit with a battery and an electrically-driven tuning-fork. When there was no initial magnetisation the sounds heard were very slight indeed. When, however, the magnetising currents were caused to circulate around the third coil, giving strong initial magnetisation, the sounds became louder. I extended the experiment until the coil became very hot, when the saturation of the core must have been well up to 15,000 or 16,000 magnetic lines to the square centimetre. I still got the loudest effect from the receiving instrument when the core was magnetised most strongly. That has a great deal to do with Figure 10, which Professor Forbes says has nothing to do with the case. I gave the figure merely as an illustration of that which we should have supposed would be the case if we were to draw our conclusions solely from the observations made, like the usual curves of magnetisation, without respect to time. Finding, as I have done, that deductions from such curves do not agree with the facts of observation, I say that there is more yet to be discovered.

Professor AYRTON: That was the point that I wished to answer when it was said that I should anticipate Lord Rayleigh's communication in the March number of the *Philosophical Magazine*.

Professor S. P. THOMPSON: I shall be very pleased to see it. I



can only repeat that, *à priori*, we should not have expected it from that curve. Professor Thompson.

I was interested in Professor Forbes's new red-hot transmitter. It is one more to be added to that rather long list which I have prepared. Professor Hughes rather took exception, I think, last time to my having mixed up together in that list the forms of instrument that were merely of scientific interest and those which were of practical value. My object was not to distinguish between the two, but to see how many they were, and to classify the various principles of action. I do not think they have ever been classified before. When dealing with abstract science it makes no difference whether they are useful or useless. Professor Forbes has classified them into two kinds—those with diaphragms which talk well, and those without diaphragms that talk abominably.

Professor FORBES: I distinctly said "legal" diaphragms.

Professor S. P. THOMPSON: Professor Hughes said that the air was a "legal" diaphragm, and it has also been said that the human body is a legal diaphragm: certainly the body is a tube, whether it is a diaphragm or not. It is a nice problem how that which is not a diaphragm at all can be made into a legal diaphragm.

I come now to Mr. Stroh's remarks. Well, I congratulate the Society very much that my paper has been the means of drawing out Mr. Stroh into giving us that most beautiful account of his further researches upon the intimate phenomena of the microphone. You may call my description of what I thought happened at the microphonic contact *poetry*, but Mr. Stroh's prose far outdoes my poetry. I have no poetry that can reach to such extremely beautiful phenomena as those that Mr. Stroh described, and I am looking forward to being able some time to have the opportunity of trying some of those effects of which Mr. Stroh told us, particularly the later experiments with the contacts in oil. I know, from experiments made long ago, that a very nice transmitter can be made by having a couple of metal plates—of gold, or platinum, or nickel—and poisoning them properly with a drop of paraffin oil at the point of contact; but I did not know

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how many very beautiful things there were to be seen with a microscope in that drop of oil. It was one of the most interesting things I have ever listened to, to hear how the little particles moved about in circulating currents, and how at the contact there was the peculiar unstable state—the transfer of particles quicker one way than the other—and how when pressed down the thing seemed to take a tone of its own for a little while and sang. No doubt we all, as I did, fully appreciated the importance of the relation between the action thus described and the phenomena of electric conduction at the points of contact. The instability observed by Mr. Stroh reminded me of that curious little piece of apparatus called “Trevelyan’s rocker,” which is a piece of brass with a groove on the under side, which, after being heated, is laid upon a block of lead, and at once begins to vibrate, wobbling on its edges of contact and singing. Professor Tyndall made researches on it years ago; it is described in his books on heat and on sound. Though I quite agree with the description given there, I differ entirely from the explanation of the phenomena that Professor Tyndall gives. He says that the effect is due to the rapid expansion of the lead; that the rocker comes down on one side of the lead and warms it, causing it to expand and hitch over the rocker again to the other side, and so on, keeping the rocker in vibration. I do not agree with that explanation, but I think that what really happens is that a sort of polarised layer of flying molecules is formed underneath, and that something very much like microphonic action is set up. The proof of that is that if you put the rocker and block in circuit with a telephonic receiver of rather low resistance you will hear it moving backwards and forwards. I suppose there is a varying thermo-electric current set up. If you put the rocker and block on a pine sound-board, and if the rocker be one that has rapid movements, you can hear its own note given out, and if while it is thus sounding anybody taps on the board you hear the ordinary microphonic tick in the receiver at the other end. I look upon it as a microphone which works under rather peculiar conditions. It transmits badly, and is a transmitter of the scientific kind, and is not one of those that are of great practical value.



Now I come to Professor Hughes's remarks. Professor Hughes Professor  
Thompson. took me to task for saying that the arrangement of microphones should be in parallel, and said that for high resistance they should be in series, for low resistance in parallel, and grouped for intermediate as with batteries. I quite agree in that view; but we do not work practically with high resistance; and, with an ordinary transformer in which the primary is of low resistance, I prefer the parallel arrangement. He also said that with my valve telephone the tube itself vibrated and acted upon the ball. My answer to that is that when you speak into the tube the voice goes up the tube and beats against the bottom of the ball; by which I get all that I require without the tube vibrating. If you put on 100 feet of speaking-tube the ball will act just as well. Is it the tube, and not the air inside, that carries the vibrations?

Professor HUGHES: Certainly; the sound goes through the wood.

Professor THOMPSON: But tubes are not made of wood, and it makes no difference whether it is a spiral wire tube lined with felt—about as dead a thing for conveying sound as possible—or any other tube; the ball acts. Again, if the ball be placed in a leaden bed, slung on a hammock of india-rubber with the end of the tube three inches away and not touching it at all, the ball talks by the air beating upon it. I tried a bed of putty, and all sorts of things, to cut off vibrations, and the experiments were quite satisfactory. If the tube be plugged with a firm plug of cotton wool, the difference between the pressure of the air on the ball when the tube is so stopped as compared with when it is open is most marked, and clearly proves whether it is the material of the tube which conveys the sound or the air inside it. I quite agree with Professor Hughes that such materials as wood or metal convey sound something like 12, 14, or 16 times *quicker*, but I do not think that therefore necessarily they carry *more*. Suppose 100th part of the sound is conveyed by the metal and 990ths by the air, which will affect the ball? Of course the larger portion, and I feel quite sure that the rapidity with which the sound goes through the metal has nothing to do with it.

Professor HUGHES: I may mention that the experiments

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about sound are due to Professor Wheatstone with his rod of wood. As for lead, it is a remarkably good conductor.

Professor THOMPSON: I am perfectly aware of Professor Wheatstone's experiments, and I have spoken through a lead wire. I am very much indebted to Professor Hughes for his clear statement that it is not the business of the designer of instruments to cure bad wires, or to cure what appears to be inherent, viz., that wires must somewhere run near other wires; but by making the instruments powerful we can get rid of stray currents and useless mutual inductions. Professor Hughes has not quite caught my meaning when I said that the path of progress lay in the direction that had been followed in the construction of the dynamo. I certainly did not mean that it was desirable to have moving parts of great inertia; what I referred to was the electrical improvement, such as eliminating eddy-currents, diminishing self-induction, and the general rule of making field magnets very powerful compared with armatures. That is, I take it, the course that we must pursue in designing telephonic apparatus. Sir William Thomson followed the same course in his investigations upon cable-signalling, and he produced the siphon recorder, which, with its big field magnet and comparatively small and light armature, is very like a dynamo, and which is very similar to what I am describing, and lies in the direction in which I hope still further to go in the design of telephones.

Now I come to a gentleman who put a whole string of questions to me—Mr. Moynihan; and I rather judged from his manner of putting them that he did not write them himself, because he did not seem to know what he was talking about. As to improvements since 1878-1885, I must refer him to what has been done by Mr. Langdon Davies with his phonopore and his open-circuit telephone, and by Graham Bell, Chichester Bell, and Sumner Tainter with the graphophone, as also by Rosebrugh and by Elsasser in duplex telephony, all of whom have done an immense amount of work since 1885. As to the number of instruments and the length of lines upon which they worked, I must refer Mr. Moynihan to the secretary of the New Telephone

Company for such information. Mr. Moynihan mentioned, truly, that Edison used thermopiles for transmitters; but Edison did not use them as I did, as they were operated by the beating against their surfaces of the neighbouring diaphragms; and although Edison said his form worked, I could never get one of them to. As to how many instruments of various forms I have invented during the last eight years, I can only inform Mr. Moynihan that it does not take long to devise a new form when experimenting (I should think I devised a hundred when experimenting with what ultimately became the valve telephone); and when one has several assistants all at work in the same direction, it is easy to get several hundreds in a very few months. In regard to the break in the Blake transmitter causing a spark and blurring sound, Mr. Moynihan could not have read slip 8 of my paper, or he would have seen that I distinctly state that in my instrument no spark occurs on account of the double differentially-wound coil in the transformer. I further said that the Blake also works splendidly when provided with the double coil. Mr. Moynihan wanted to know whether there were not microphones in my transmitters. I thought that was self-evident. Mr. Moynihan told me—what I knew before—that working microphones in parallel was old, and he read out some extracts from a paper on that subject by Mr. Bidwell. If he had only read slip 4 of my paper he would have seen that I referred to Bidwell's work as being known to me; and being so known I had no wish to take up the time of the Society by going over what was old. Mr. Moynihan declared that the diaphragm of the Blake transmitter was so thick that it could have no tone of its own. Well, that is his opinion; but the fact that there is an india-rubber damper put against it does not quite bear out that opinion. Perhaps Mr. Moynihan thinks that the damper is put there for show. I, on the other hand, think that the presence of the damper proves my point. Mr. Moynihan wanted to know what an induction plug was. If he had read slip 8 of my paper he would have seen that I use an electro-magnet as an induction plug. I thought an electro-magnet was a plain and well-understood term, and I do not know whether Mr. Moynihan

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wished to score a point for my having invented a new term when I used the word "electro-magnet." Mr. Moynihan surely cannot be ignorant of the fact that an electro-magnet is in a sense opaque to the passage of induction currents. Varley took out a patent in 1870 for a sort of telephone and telegraph, in which he used for the same purpose an electro-magnet, which he called an "echocyme," because it was opaque to the current; Van Rysselberghe used the same, and called it a "separator." I do not quarrel with them for using those terms; but I said that I used the electro-magnet as an induction plug, which description I consider equally applicable, and, in my opinion, preferable.

Now I come to Mr. Preece. I am told that Mr. Preece has been detained by important engagements and cannot be present this evening, but I do not doubt that he wishes me to answer his remarks just as if he were here. Well, it is really extraordinary that I have survived the terrible criticism which my paper received at the hands of Mr. Preece; but the truth is that we have, as he told you, fought before, and he knew beforehand that I should return his attack; he knew it and expects it.

We all know how perfectly sure—in fact, cocksure—Mr. Preece is when he forms opinions or makes statements. He was cocksure that the early forms of telephones were only toys; he went to America cocksure that he would in a quarter of an hour expose Graham Bell. He was cocksure, at least when he spoke the first time, that my theory of long-range telephony was wrong. Now it may be admirable to be cocksure, but it is not scientific. I am told that I am expected to wind up this debate by replying to Mr. Preece; but what is there left to answer? Not much; for Professor Hughes has answered Mr. Preece on the question of designing instruments, Mr. Stroh on the question of the phenomena at the contacts, Professor Ayrton on the question of theory. Indeed, I feel sure that Mr. Preece himself felt that what he said at first was capable of being answered; for, as you will remember, on the second occasion when he spoke he executed two remarkable strategical movements: in the first place he buttered, and in the second place he hedged. It is true *that his butter was of a rather cheap description—in praise*

of my books on other subjects. His solicitude for my reputation was quite touching; it even exceeded his care for his own. But I was truly amused when I found Mr. Preece on the second occasion making careful preparations to come round to my views. What did he then say? "The notion of getting over this difficulty by making the receiver weak and the transmitter strong is one that has occurred to everybody's mind since the very earliest days of telephony. It was communicated in his [Mr. Preece's] first paper read before the British Association, in 1877; it was the basis of the discoveries and work of Edison; it had even been carried out on the London and North Western Railway, where Mr. Fletcher had weakened the receiver and strengthened the transmitter." Further on Mr. Preece said that he agreed with me if it were possible to diminish the sensitiveness of the receiver and still speak; but he added: "We can only diminish the sensitiveness of a telephone receiver to a very small extent, and then we get beyond the limits of the human ear." Well, I can only reply that the limit of sensitiveness of the human ear has nothing to do with the question; for if we first make the receiver one-tenth part as sensitive as it is, and then make the transmitter ten times as powerful as before, the loudness of the received speech will remain as loud as it now is, while the induction noises will be diminished by 90 per cent. Is that nothing to be desired? But the "if" that Mr. Preece introduced shows that his opinion is decidedly coming round toward mine. He now says that he put forward the same ideas in 1877. Well, I have looked up the Report of the British Association for 1877 (page 36), and this is all I can find:—"The transmitter currents are too weak, and Mr. Edison, of New York, has endeavoured to remedy those defects in Bell's [transmitter] by introducing a transmitter which is operated on by a battery current. . . . Starting from Reis's transmitter, he simply substitutes for the platinum point a small cylinder of plumbago. . . . The interference of working wires will seriously retard the employment of this apparatus; but there is no doubt that scientific inquiry and patient skill will rapidly eliminate all practical defects." What I have said is that the proper thing

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was to make the transmitters more powerful and drown out the induction sounds, and there is a great difference between that and what Mr. Preece said in 1877.

As to my theory of microphonic action, on the first occasion Mr. Preece described it as mere poetry, and on the second occasion he described it as being his own theory of 1882. I have looked up the Report for 1882, and this is what I read as having been said by Mr. Preece\*:—"In 1877, at Plymouth, I had the pleasure of showing in actual operation the finally developed instrument now known as the Bell telephone, which I had just brought over from America. . . . In 1877 it was a scientific toy; it has now (1882) grown to be a practical instrument. . . . There is now little doubt that it [the action of the microphone] is due to effects of heat generated by the passage of electricity between two points in imperfect contact, whose relative distance is variable. Carbon is the best material for the purpose . . . because it has the remarkable property of having its resistance lowered when it is heated—the reverse of metals. . . . This observation is due to Mr. Shelford Bidwell."

So I find that on January 27th, 1887, Mr. Preece said that my theory was mere poetry; on February 10th, 1887, that it was his own theory; and on August 28th, 1882, he there says it was Mr. Shelford Bidwell's theory! Which of these three statements am I to believe? which of them am I to answer? I can only conclude that they are all three wrong—that my theory is not poetry, that it is not Mr. Preece's, and that it is not Mr. Bidwell's. According to Mr. Preece, in 1882 Mr. Shelford Bidwell stated that he had discovered—what was pretty well known before—that the substance known to electricians as carbon changed its specific resistance when heated, but, unlike metal, which increased, carbon decreased in resistance; and he concluded from that that it had something to do with the microphone. It is rather a lame conclusion, but that was Mr. Preece's conclusion.† I have pointed

\* *Electrician*, vol. ix., p. 389.

† *Note added March, 1887.*—The conclusion arrived at by Mr. Preece in 1882 does not quite represent Mr. Bidwell's conclusion. Mr. Bidwell had pointed out that pressure on contacts both of metal and of carbon increased the current



out to you that what goes on at the contacts has absolutely nothing whatever to do with the specific conductivity of the materials; it is a *kinetic* phenomenon rather than a phenomenon of *conductivity*. You can make a good transmitter out of gold, platinum, or hard carbon, or you can get bad conductors, such as lamp-black, which do not work so well; it is a question of surface contact, not of what is a good or a bad conductor. My account of the intervention of heat in the molecular phenomena at the contacts is totally independent of anything that heat may or may not do to the conductivity of the materials. I believe my theory to be new, and to be true so far as it goes.

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I now pass to the points of abstract theory laid down by Mr. Preece, and from which I utterly and entirely dissent. His theory is not only mathematically wrong in itself, but is founded upon wrong assumptions. What does Mr. Preece assume? In the first place, that a telephonic line is to be considered as a submarine cable; that, therefore, the capacity and the resistance are the only things to be taken into account. I, on the other hand, hold that in a telephone line the only important things to be taken into account are the mutual induction, the self-induction, and the resistance; the capacity being negligibly small. Professor Ayrton has emphasised this point already, and has pointed out the essential difference between the static retardation of signals in a cable due to its capacity, and the retardation of phase in telephonic signals due to self-induction. The laws of the two are absolutely different; and I have no doubt as to whether my assumptions or Mr. Preece's are the more correct. Take one case—that of underground telephone lines—in which you get the nearest approach to cable conditions: why,

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and decreased the resistance; but that whereas in the latter case the increase of current tended to produce a further decrease of resistance by its heating effect, in the former case the increase of current by its heating effect tended to diminish the decrease of resistance. I fail to see that this has any real bearing on the case. It might be important if it were true that metallic contacts will not transmit speech; but in any case it is not the same conclusion that Mr. Preece propounded in 1893, neither does it in the least degree anticipate my *kinetic* theory of the *microphonic* contact.—S. P. T.



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Mr. Preece himself says that in underground lines you can talk further with copper than with iron wires. There is proof positive that self-induction is not the insignificant factor that Mr. Preece would have us believe when he says that capacity and resistance are the only things to be taken into account in calculating the possible length of line for transmission. Well, having made this altogether erroneous assumption that a telephonic line is, for purposes of calculation, the same thing as a cable, he then goes on to quote Sir William Thomson's law about the square of the length of the cable, which law, he said, was as true as Ohm's law. Of course it is as true as Ohm's law for every case where it is applicable; that is to say, for every case where capacity and resistance are the only important things, and where mutual induction and self-induction may be left out of consideration, and where also—and this is important—the assumption may rightly be made that the turning on of the signal current is an absolutely sudden single act. But no; this is not good enough for Mr. Preece. He claims a much more sweeping application of this law. According to Mr. Preece—and he seems to think that when he says that he was the assistant of Latimer Clark and of Faraday that that settles the matter—"the law that determines the transmission of currents through a wire to produce telephonic signals is precisely the same in every respect as the law that determines the flow of currents through submarine cables, or, indeed, through any circuit." Unfortunately the statement is untrue. Further, Mr. Preece said that the number of currents that could pass through a wire (he said wire, but I think he meant a cable) was "not in any shape or form dependent upon the electro-motive force." Well, even admitting the truth of this rather doubtful statement, the question is not how many currents will pass, but how many can be made audible. Suppose the number of currents transmitted per second to be very great, and that each impulse got so attenuated as to be all but inaudible, and that then you increase the electro-motive force just so much as to make them audible: you are not increasing the number that pass, but you are increasing the number that can be heard. Mr. Preece further

mixed up in a most deplorable way the speed of transmission of a current with the rate of signalling of currents. The speed of transmission has nothing whatever, *per se*, to do with the distance to which speech is possible, or with the frequency of the vibrations. If you put in 100 vibrations per second at one end they come out 100 vibrations per second at the other end, not 99 or 98 or any other number; they come out 100 per second whether they have been retarded or not retarded in transmission.

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But, taking as a basis of argument Mr. Preece's own ground—that capacity and resistance are the only elements of importance—I still maintain that it is incorrect to say either that the retardation is proportional to the square of the length of the cable (or line), or that the signalling does not depend on the electro-motive force. Remember that Thomson's law assumes that at the sending end the wire is raised suddenly and abruptly from zero-potential to a finite potential—call it  $V_0$ —and maintained constant at that potential for a definite time while current rushes into the cable in a sort of electric wave which flattens itself out and expends itself in charging the cable as it goes along. From this assumption, and the mathematical laws of Fourier, Sir William Thomson calculated the value of the potential at a given distance  $x$  along a cable of length  $l$ , at a time  $t$  after making contact, to be expressed as follows:—

$$V_{(x,t)} = V_0 \times A e^{-\frac{Rt}{kr l}},$$

where  $k$  and  $r$  are the capacity and resistance, respectively, of unit length, and where  $A$  and  $B$  are quantities depending on  $x$  and  $l$ , and on the fraction of the whole potential to which the potential at  $x$  will have risen at the time  $t$ . Hence, if the fraction of potential to which the point  $x$  is to rise be given as constant (by the construction of the telegraphic receiving instrument, which requires a certain finite potential applying to work it), it follows that the time required to get a complete signal through is proportional to  $kr$  and to  $l^2$ .

Now in telephonic work all this is different. We do not work with receiving instruments that refuse to give any indication (such as printing a mark or making a sound, or even visibly

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deflecting a spot of light) until the potential applied to them has risen to a certain definite amount which it is useless either to exceed or to fall short of. The telephonic receiver has to respond to all sorts of minute impulses. Again, the telephonic sending is utterly different from the single sudden raising of the potential to a fixed amount, as assumed in the above argument. It is much more nearly true to assume that the potential varies periodically in a harmonic function of the time. Let  $n$  stand for the number of vibrations per second that are to be transmitted; then  $V_0$ , the potential at the sending station, will be represented by an expression of the form

$$V_0 = A \sin. 2 \pi n t,$$

where  $A$  is the amplitude of the alternations of potential, or, in other words, the maximum potential at the transmitting end. Then for the potential at the distance  $x$  along the cable we shall have

$$V_{(x,t)} = A e^{-\sqrt{\frac{n}{k r}} x} \sin. (2 \pi n t - \sqrt{\frac{n}{k r}} x).$$

From this we may deduce that the velocity of wave-propagation will be equal to  $2\sqrt{\frac{n}{k r}}$ , and therefore depends, in the first instance, upon  $n$ . This is utterly unlike anything in Thomson's law, which Mr. Preece preaches as being the whole gospel. Further, we see that if the above be the speed of wave-propagation, the time taken by the waves to travel a definite length of cable is not proportional to  $k r$ , nor to  $l^2$  (as Sir William Thomson's law shows for telegraphic signals), but is proportional to  $\sqrt{k r}$  and to the length simply.\* The law of the square of the distance about which Mr. Preece makes such a fuss is not true for any simple sound telephonically transmitted. Lastly, it is obvious from the equation that for any set of consecutive vibrations the potential at the receiving station is proportional to  $A$ , per amplitude of the alternations of potential at the sending station; proving the falsity of Mr. Preece's argument that the electric

you are not <sup>idled</sup> March 12th.—Dr. J. Hopkinson, F.R.S., tells me that some increasing the <sup>set</sup> this case as an exercise in the Mathematical Tripos at he was examiner —S P T.

increasing the electro-motive force of transmission has no effect.\* Pr  
Th  
Why, if increasing it has no effect, then diminishing it ought to have none! Let us signal without any electro-motive force at all! I should recommend Mr. Preece for the future to avoid mathematical arguments. My only reason for replying to his extraordinary statements about the mathematical theory is that I do not let them pass unchallenged for fear that other members of the Society might otherwise think that Mr. Preece's ideas on this topic were either authoritative or intelligible.

Returning from Mr. Preece's theory to his opinions about telephonic practice, I note that Mr. Preece is cocksure that the whole of the difficulties that I have mentioned as occurring with telephones in practice are imaginary. When he said so I wondered what sort of telephones Mr. Preece must have been used to. What awfully good Blake transmitters he must be in the habit of using if they never require adjusting, and if they don't give out a harsh, rasping sound when you speak too loud or too near! For myself, I still take the more modest view that, in London at least, the average telephonic instrument is capable of improvement, in spite of Mr. Preece's assurance that everything is as perfect as it can be.

But the thing that seemed to have excited Mr. Preece most was what he was pleased to term my "perversion of history." Now "perversion of history" is an ugly charge for one man to make against another; and if I had not known that it was only Mr. Preece's way of saying that his version of history differed from mine, I might have taken serious notice of it. According to Mr. Preece I was both unjust and ungenerous in daring to name Reis, and Varley, and Wray in the same line with such men as Graham Bell and Edison. I was ungenerous in

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\* *Note added March 13th*—I may also add that Kirchhoff has discussed the question from a somewhat similar point of view. From his investigations we may deduce the value of the telephonic current as

$$i = \frac{A \sin. (2\pi n t - \phi)}{R} \sqrt{\frac{\bar{b}^2}{\cosh^2 b - \cos. b^2}}$$

where

$$b = 2\sqrt{2\pi n k r l^2}.$$

These expressions afford the same conclusions as before.—S. P. T.

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comparing Bell, and Edison, and Professor Hughes with men who—according to Mr. Preece's version of history—were only playing with a musical toy. Mr. Preece went on to say that before Sir William Thomson brought back Bell's telephones from Philadelphia in 1876 nobody in this kingdom—I believe he said in Europe—had conceived or even heard of the possibility of transmitting speech electrically. Well, Mr. Preece speaks for himself, and possibly he had not. He was cocksure that it could not be done, and therefore the thing was inconceivable; no one could have heard of it. Nevertheless, he must be now aware that Reis in 1861 set out with the deliberate object of constructing an instrument for transmitting speech with vowels and consonants amongst its articulate tones, and that transmission of music was an afterthought. If he doubts it, there is the "Jahresbericht" of 1861, in black and white, in the British Museum Library, to prove it. He must know that Yeates had transmitted speech in Dublin, and Yeates is only one of many. Nay, I myself, a whole year before the date assigned by Mr. Preece, had been shown a Reis's telephone as an instrument intended to transmit speech. It is just as untrue to assert that Reis's and Yeates's instruments were mere musical toys as it would be to declare that Gramme's early dynamos were only toy motors. Would that be just or generous, or even true, when we know that Gramme invented them as dynamos, and that their use as motors was an afterthought?

It is all very fine to denounce Reis's telephone as a mere musical toy. How about Graham Bell's original patent (U.S.) of March, 1876, in which there is not a line, not a word, not a syllable, about speech; plenty about tones and notes, but not a hint of a vowel or a consonant? Mr. Preece knows that as well as I do; yet he has the impudence to decry Reis's instrument as a *musical toy*! Take the names of the brothers Wray, the mention of which appeared particularly to anger Mr. Preece. They had spooled the transmitter—improving it, no doubt they thought—by substituting for the loose contact of Reis a mercury-cup arrangement; but they had invented a new kind of receiver with two electro-magnets mutually attracting. If they had merely adapted some old and well-known telegraphic receiver—for

example, a polarised relay—for their telephone receiver there would have been no invention worth mentioning. But they invented a new instrument for a specific purpose. Are we to deprive them of the credit of their improved receiver because of their bad transmitter? They only succeeded in transmitting half musical notes: that was not the fault of their receiver. If it were true that the best receiver for music is not the best receiver for speech, or that the best receiver for speech is not also the best receiver for music, I could have understood Mr. Preece's anger. But the instrument that is best for speech is also best for music, and therefore I am justified in crediting the Wrays with the good work they did, though it happened to be done at a time when Mr. Preece was satisfied that speech never had been and never would or could be transmitted. It would surely be ungenerous and unjust to the Wrays to ignore their work. But my offence seems to have been that I distorted or perverted history by daring to name Reis and Varley in the same line as Bell and Edison. Well, if Graham Bell had lived fifteen years before Reis; if Bell had worked on for years, neglected and unknown, despised and rejected by the complacent telegraphic engineers of his time, and if before he had been two years in his grave his great invention had been proclaimed as some one else's, then I could have understood that I had been unjust to Bell to name Reis in the same category. But it was Reis who lived fifteen years before Bell; Reis whose invention was tossed aside as a toy; Reis who died heart-broken. Talk of injustice in naming alongside of Reis those who have made fortunes out of that which he invented! The injustice does not lie there. As to the exquisite invention brought out by Graham Bell in 1876, when he showed that you could take an instrument like a polarised relay and make it talk to a similar one at the other end of the line, that was a subsequent and a very beautiful discovery. Nothing shall ever cause me to detract from the merit of this discovery of Bell; only he was not the first, and his invention has almost ceased to be used for transmitting speech. We have returned to the battery transmitters invented by Reis and perfected by Edison, Hughes, Blake, and others.



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I have been told—and if it is so no one more deeply regrets it than I—that in the delivery of my paper (which I did not read *verbatim*) I seemed to be ungenerous in my references to Professor Hughes. Now I yield to no man in my admiration for Professor Hughes and of his researches: the world knows them; they need no praise of mine. I did commit the mistake of crediting to Mr. Preece the announcement of the discovery—which I am assured was made really by Professor Hughes, and merely announced by Mr. Preece—that the Hughes microphone would work if placed on a person's throat or chest, or in his pocket. The discovery that a microphone will work without any diaphragm is an important one, and its importance was duly recognised at the time. I apologise most sincerely to Professor Hughes for having so read the reports of the meeting of this Society where this occurred.

But I have not yet done with this alleged injustice. The charge that I have dared to name in the same line such men as Reis and Varley with Graham Bell and Elisha Gray is a curious one, coming from Mr. Preece. He may not know that I had authority for doing so—at least, I only followed the example of one whom I used to regard as an authority. To make my meaning plain let me quote from an authoritative document, filed in H.M. Patent Office on 30th of November (No. 2202 of 1878, page 2, line 18). It is a specification of an invention entitled “Improvements in Telephones”:—“This invention relates to improvements in telephones. . . . In Riess's, Gray's, Varley's, Bell's, and other known forms of telephone, these sonorous vibrations are reproduced directly by the action of electricity, and are reinforced by simple sounding-boards or resonators. Now according to this invention the vibrations produced in the receiving instrument are transferred from their source to fixed or stretched membranes or discs in such a way as to increase the quantity of air thrown into sonorous vibration. . . . The receiving instrument will consist of a polarised relay, whose armature, vibrating in unison with the original source of sound, will, by its connection through a wire with a drumhead similar to that of the toy telephone, reproduce the transmitted sounds, whether articulate



or otherwise." The inventor who thus writes,—who thus dares to place in one line the names of Reis (which he misspells), Gray, Varley, and Bell,—who speaks of their instruments as known telephones,—who contrasts these well-known electric telephones with "the toy telephone,"—is "Mr. William Henry Preece, of Wimbledon, in the County of Surrey, Civil Engineer."

Mr. Preece was equally unfortunate in his other instance of my "perversion" of history. I had said, and I am quite ready to repeat it, that Van Rysselberghe's much-applauded methods for the perfection of long-distance telephony are a mere commercial extension of the researches of Black and Rosebrugh made in 1878-9. Whereupon Mr. Preece says that Van Rysselberghe has been roughly handled by being bracketed with Black and Rosebrugh, and hurls at me the crushing rejoinder that Van Rysselberghe's object was not to obtain long-distance telephony at all, but was to work telephones simultaneously with telegraphs on the ordinary telegraph lines. It is well to be sure of one's facts. I am generally sure; Mr. Preece is cocksure. I have before me the patent specification of Black and Rosebrugh. Mr. G. Black is not a nobody; he is the present manager of the Great North-Western Telegraph Company of Canada, at Hamilton, Ont. Dr. Rosebrugh is a well-known physician at Toronto. Their United States patent is dated February, 1879. Their British patent, taken out on April 16th, 1879, says (page 1, line 9):—"The object of this part of the invention being to enable telephones to be connected with the wire of any galvanic circuit without interfering with the action of said galvanic circuit. . . . The invention consists, secondly, in connecting two or more wires of a telegraph line running in the same direction and using them as a conductor, by which arrangement . . . the length of line on which a telephone can be used is very much increased." And on page 3, line 15, they add that thus they can use the wires that are being used for ordinary telegraphic purposes also. The details of the specification show that in 1879 they were even using exactly the same means as Van Rysselberghe did in 1882, namely, that they used condensers to block the telephone circuits against the telegraphic currents, and employed the inertia of the electro-magnets in the

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telegraph instruments to block those instruments against the telephone currents! And yet, forsooth, I am told that I have perverted history!

I have no doubt that when in 1879 Mr. Preece heard of the experiments of Black and Rosebrugh in using telephones simultaneously on the Canadian telegraph lines he treated the whole thing as a *toy*—was cocksure that the thing was absurd. It was not till 1882—when Van Rysselberghe's reinvention of the same notion was noised about, and exploited by wealthy companies, and became the subject of Government concessions and of reports by professional experts—that Mr. Preece could see anything in it. But fame and wealth came to Van Rysselberghe. Mr. Preece executes a *volte-face* as shamelessly as he did over Bell's telephone, and now accuses me of perversion of history. He has mistaken his man this time.

I now come to some minor points in my long list of sins.

Mr. Preece objects to my adoption of the word *transformer*, and tells me that "induction coil" is good enough. Well, I did not invent the word *transformer* (I believe Edison used it first), but it is an admirable term. It is true that some transformers are induction coils. It is also true that a *relay* is no more than a piece of iron or steel with some coils of wire on it; but I prefer to call it a "relay:" that term expresses its function in a system. Time was when the relay was always called "the receiving electro-magnet." "Relay" has survived. "Transformer" will survive too, in spite of Mr. Preece. I am not so sure whether the *kilowatt*, that Mr. Preece has tried to introduce instead of the *horse-power*, will survive.

Then Mr. Preece objects to my use of the term "quasi-metallic." I did not invent the term, and it is sufficiently distinctive. Curiously enough, Mr. Preece seemed to think that a body possessing quasi-metallic conductivity must necessarily be a semi-conductor. Indeed, I think he was quite sure on the point. Really I did not suppose that at this time of day it was needful to enlighten Mr. Preece that conductors may be divided broadly into two classes—one comprising those which conduct, like metals, without decomposition; and those which conduct with

decomposition, and which are called electrolytes. And, further, Professor  
Thompson. that there is a group of bodies which, though not metals, conduct like metals—amongst them being such substances as phosphor-bronze, iron pyrites, retort carbon, galena, sulphide of copper, selenide of copper, magnetic oxide of iron, and so forth. These bodies, being not electrolytes, and not metals, conduct as metals, and therefore possess quasi-metallic conductivity. Neither are they the same thing as semi-conductors. Amongst the substances classed by the authorities as semi-conductors are powdered glass, flour of sulphur, and dry ice: I have yet to learn that any of these possess quasi-metallic conductivity. Moreover, most of the substances I have named above as having quasi-metallic conductivity are excellent conductors. Retort carbon is a good conductor. Faraday singles out sulphide of copper in particular as being a good conductor.

Still more extraordinary was Mr. Preece's position respecting patent questions. After very carefully pointing out—what I myself have definitely stated—that the essential part of the majority of the transmitters described by me is a microphone, Mr. Preece then turns round and insinuates that my instruments have been so designed as to avoid infringing somebody's patents. Well, that is a very curious insinuation to come from Mr. Preece. What does Mr. Preece want? Does he think it a merit in an inventor when he invents things that are infringements? Is it a crime to invent things that do not infringe? I expect to be abused next for not having stolen Mr. Preece's watch. Did Professor Hughes patent his microphone? On the contrary, everyone knows the generosity—the ill-advised, ill-requited generosity—with which that splendid invention was given to the world. Thanks to Professor Hughes's generosity, I am as free to use a microphone in my instruments as I am to use a binding-screw or a speaking-tube. Surely Mr. Preece must have forgotten his brief when he abused me for the fact that my instruments do not infringe anyone else's patents. He talked rather wildly about a patent being a property that ought to be protected. Protected for what? To make it an instrument for grabbing other inventions that are not included in its claims? Is that

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the new morality of which Mr. Preece is the devoted advocate? If a man who is not the first inventor of telephones takes out a patent for one kind of electric telephone, is that patent to be afterwards so stretched and broadened out that it is to include all other kinds of electric telephones, including those that are expressly excluded by the inventor's own specification? If that is Mr. Preece's meaning, then I can only ask you to listen to these words, which were uttered in this very room on the 23rd of March, 1882\* :—"If the power of conversation by the variation of an electric current is to be made a monopoly, then there must be something very rotten in our patent laws, and something that urgently requires reformation." The speaker on that occasion was Mr. William Henry Preece.

Now, lastly, I come to Mr. Preece's extraordinary views as to the work of long-range telephony. He told us that it wasn't instruments, but the environment of the lines, that must be altered. He told us about his experiments—how he had talked on a special line some 270 miles long, across the South Midlands, with greater ease than from St. Paul's to Westminster. It sounded very fine; but what was the practical value of the experiment? He went down to Hanwell, and talked thence to Nevin. Who wants to talk from Hanwell to Nevin? There is, I believe, a lunatic asylum at Hanwell; what there is at Nevin I do not know. I believe it is an obscure village in South Wales. If getting rid of environment means that we shall only be able to talk from Hanwell to Nevin, and not be able to talk from the heart of London to the heart of Birmingham or of Manchester, then the less said about it the better. Mr. Preece appeared to have got environment on the brain. To get rid of environment is simply absurd, unless we are prepared to pull down all telegraph lines, all electric bells, and all electric lights, to make way for the telephone. To sum up the matter: Mr. Preece says that the successful transmission of speech by telephone depends on the environment of the wire, not on the instrument; and he supports his view by alluding to cases where, conditions (which would be

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\* *Journal of the Society of Telegraph-Engineers*, vol. xi., p. 146 (1882).

impossible in ordinary practice) being entirely favourable, speech Professor Thompson. was transmitted by ordinary instruments; and he concludes, therefore, that special instruments are not required. My argument, on the other hand, was that the environment being in practice entirely unfavourable, as acknowledged by Mr. Preece, I propose to use instruments which will enable me to neglect and disregard the effect of the environment. I can only say that I am quite willing to await the verdict of posterity as to the issue between us.

The PRESIDENT: We have already passed a hearty vote of The President. thanks to Professor Thompson for his paper, and I am sure that you will all agree that he has favoured us with a very able reply to the very full discussion upon his paper. We wish him success with his telephones.

A ballot for new members took place, at which the following were elected:—

*Member:*

Dr. C. Baur.

*Associates:*

William Henry Butler. | Philip Jolin.

*Students:*

Edwin Percival Allam. | Albert Ernest Richardson.  
Robert William Paul. | Stanhope Evelyn Thornton.

The meeting then adjourned.

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## A BSTRACTS.

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### H. L. FRENCH—A RELATION BETWEEN MAGNETISING FORCE AND CORE OF MAGNET.

(*Electrician and Electrical Engineer*, Vol. 5, No. 60, Dec., 1886, pp. 445-46.)

It is of interest to know the greatest number of ampère-turns that can be used economically on a given core. The currents for several cores of gradually increasing size were first ascertained; the highest economical currents were then plotted for each magnet, and a final curve was drawn, taking these highest economical currents as abscissæ and the diameter of the cores as ordinates. The simplified equation for this curve, giving the relation between the current and the diameter, is

$$y = 9x^4 - 26x^3 + 22x^2 - 0.7x - 0.22.$$

Having thus found the current for a given core, it must be multiplied by the number of turns to get the magnetising force.

The horseshoe electro-magnets experimented upon varied in diameter from 1.22 cm. to 6.34 cm., the length of each limb being about 20.8 cm. Above each bobbin on the electro-magnets, a small space was left, under the armature, in which a narrow coil of fine wire could be placed. The electro-magnets were excited by an accurately measured current from a dynamo. The narrow coil of fine wire was connected to a galvanometer, so that on reversing the main current the induced current in the narrow coil could be measured. From this the strength of the magnet is arrived at, as it is proportional to the sine of half the angle of deflection.

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### DEBRAY — REPORT OF THE CHEMICAL SECTION OF THE ACADEMY OF SCIENCE ON MR. MOLSSAN'S EXPERIMENTS FOR THE SEPARATION OF FLUORINE.

(*Comptes Rendus*, Vol. 103, No. 19, Nov. 8, 1886, pp. 850-60.)

The question of the separation of fluorine is one which has long occupied the attention of chemists, though up to the present time all the experiments made have been attended with very little success, owing to the extraordinary chemical energy of fluorine, which causes it to combine with almost every other body.

Davy first tried to separate fluorine by the electrolysis of hydrofluoric acid, he found that his platinum electrode was attacked, but he was never able to separate the active agent. Fremy also tried to separate fluorine, by decomposing by a current various anhydrous metallic fluorides, but with little

success. His experiments of the electrolysis of fluoride of potassium were more successful, as he noticed the production of a gas which decomposes water and replaces the iodine in iodides; this gas he concluded was fluorine.

Mr. Moissan, after attempting unsuccessfully the separation of fluorine by chemical reactions, took up again the process of the electrolysis of hydrofluoric acid. The acid was condensed in a U tube of platinum placed in a bath of methyl chloride kept always at a temperature less than  $-23^{\circ}$ , which if necessary could be reduced to  $-50^{\circ}$ . The ends of the platinum U tube were closed by plugs of fluorspar, through which passed the platinum electrodes, while two lateral tubes gave passage to the gas evolved. Anhydrous hydrofluoric acid being a bad conductor, a small quantity of potassium fluoride was added. The current from 20 Bunsen cells was allowed to pass for two or three hours, and it was found that hydrogen was given off regularly at the negative electrode, while at the positive electrode there was evolved a gas which attacks metals with the formation of fluorides, decomposes water, combines with phosphorus so violently as to set it on fire, sets fire also to silicon with the production of fluoride of silicon, and violently attacks most organic compounds. This gas may be fluorine or perfluoride of hydrogen, or a mixture of hydrofluoric acid and ozone. The last hypothesis is untenable, as it would necessitate the presence of water with the hydrofluoric acid. The second is also rejected, because the gas produced at the positive pole is entirely absorbed by iron, forming fluoride of iron, without leaving any trace of hydrogen. The gas evolved by the electrolysis of hydrofluoric acid is therefore really fluorine.

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#### G. A. CASSAGNES—STENO-TELEGRAPHY.

(*Comptes Rendus*, Vol. 103, No. 24, Dec. 13, 1886, pp. 1190-3.)

This is a direct application of the mechanical shorthand-writing apparatus to telegraphy. At the sending station is placed the mechanical stenographic instrument, by means of which the operator, always following the words spoken, can punch a row of holes in a paper slip, arranged in small horizontal lines, each of which represents at least one syllable. The perforated slip is placed under an automatic transmitter, and the current passes through the punched holes to line through a distributor. At the receiving station the current arrives at a receiver, which is a counterpart of the distributor at the sending station, with which it moves synchronously. The several contacts of the receiver are in connection with a series of polarised relays, equal in number to the keys of the stenographic instrument, which set in action the printing apparatus; the row of holes at the sending station is thus transformed into a row of marks at the receiving station. The paper slips then each advance a step, and the apparatus prints another row of marks.

Experiments on a single wire have shown that up to 318 miles, 400 words a minute can be transmitted; up to 400 miles, 280 words a minute; and up to



560 miles, 200 words a minute. Not only does this apparatus increase the rapidity of transmission, but by its aid a speech may be taken down in writing at a place hundreds of miles distant from the speaker.

### G. BERTON—EFFECT OF TEMPERATURE ON MAGNETISATION.

(*Journal de Physique*, Vol. 5, Oct., 1886, pp. 437-56.)

A bar of a magnetic metal was placed in a magnetic field and brought successively to various temperatures; the total magnetic force was determined for each, and then, the magnetic field having been annulled, the permanent magnetism at the same temperature, the difference giving the temporary magnetism.

The magnetic moment was determined by Gauss's method. The magnetic field was produced by a coil specially made, consisting of a glass tube just large enough to admit the bar, on which was wound a bare copper wire, having the spaces between the turns greater than the diameter of the wire. The glass tube had flanges at each end, so that on being placed inside a metal cylinder, which fits the flanges, the copper wire does not make contact. The metal cylinder was plunged into a bath of paraffin, which could be heated to any desired temperature. The temperature was read by a thermometer placed inside the coil and touching the bar to be experimented upon.

The deflection by the Gaussian method is first determined, once for all, for the coil alone without any core; then the bar is introduced, and two magnetising currents are sent through the coil, each being reversed, so that four deflections are noted; finally, the deflection for the bar alone without any current in the coil is determined.

For iron it appears that the total magnetisation is almost independent of the temperature, at least within the limits of the experiments, which were continued up to  $341^{\circ}$ . It seems, however, to increase very slightly at first, and to reach a maximum at about  $300^{\circ}$ . The magnetic moment of a bar 7.58 cm. long and 0.535 cm. in diameter was 85.55 at  $37^{\circ}$ , and 36.1 at  $292^{\circ}$ .

Nickel shows much more marked changes. The total magnetic moment of a cylindrical bar increases with the temperature up to about  $200^{\circ}$ , and then decreases continuously; above  $290^{\circ}$  the decrease becomes very rapid, and the moment becomes nil for a temperature below  $340^{\circ}$ . The residual magnetic moment constantly decreases as the heat is increased, and becomes nil at  $350^{\circ}$ . The temporary magnetic moment begins by increasing, and reaches a maximum at about  $250^{\circ}$  or  $260^{\circ}$ , and afterwards becomes nil.

With cobalt all three magnetic moments continually increase with the temperature, as far as the experiments have gone, i.e., to  $321^{\circ}$ .

Experiments on tempered steel show that the total and temporary magnetic moments increase with the temperature at least as far as  $335^{\circ}$ ; the residual magnetism, on the contrary, decreases very slowly but continuously. The magnetic moment of steel bars may be considerably increased by magnetising when hot and hardening them at the same time; thus a hardened

steel bar magnetised when cold had a magnetic moment of 25.4, but when magnetised hot and hardened at the same time the magnetic moment for the same magnetic field was 49.45. In a second experiment at 17° and at 240° the values were 18.35 and 43.35.

The behaviour of nickel having been especially remarkable, further experiments were made on nickel needles to determine the coefficient  $k$  of magnetic susceptibility, and the coefficient  $\mu$  of magnetic permeability, for various temperatures.

### **T. CALZECCHI-ONESTI—CONDUCTIVITY OF METALLIC FILINGS.**

(*Journal de Physique*, Vol. 5, December 1886, p. 573. *Il Nuovo Cimento*, Vol. 17, pp. 38-42.)

A glass tube containing filings is placed in a voltaic circuit; its conductivity is found to be nearly nil, but on passing through the filings the induced currents from an induction coil the filings acquire a certain degree of conductivity which they afterwards preserve, but which disappears little by little in time. The value of the conductivity depends essentially on the chance arrangement of the metallic particles.

### **A. ROITI—ELECTRO-CALORIMETER.**

(*Journal de Physique*, Vol. 5, December 1886, pp. 576-79. *Il Nuovo Cimento*, Vol. 17, pp. 1-12.)

The electro-calorimeter is constructed with two spiral Breguet springs placed one above the other, the one with the silver inside, the other with the silver outside, they are fixed by their opposite ends, A B<sup>1</sup>, to terminals which bring the current, joined together at their free ends, B A<sup>1</sup>, and at about the middle C of the height of the apparatus by a small rod placed in their common axis; the expansion of the two springs tends to rotate in the same direction a metallic pointer or a mirror fixed to the springs at C. The arrangement, insulated by two discs of ebonite, is enclosed in a brass tube pierced with a number of holes; this tube is surrounded by a double case of large size, containing water, and intended to cut off all radiated heat from outside objects. The movements of C are observed by Poggendorff's method.

The author gives the mathematical proof that the rise of temperature of the springs is proportional to the quantities of heat they receive, contrary to the opinion expressed by Lenz and Poggendorff. This theoretical proof is supported by the results of experiments in which the electro-calorimeter was compared with an air thermometer. The results agreed very closely; and it is worthy of note that the electro-calorimeter gave results just as accurate for alternate currents as for continuous currents. The instrument was of great use in determining the efficiency of the secondary generators of Gaulard and Gibbs,

**A. M. TANNER—SIMULTANEOUS TRANSMISSION OF MESSAGES  
BY ONE LINE WIRE.**

(*La Lumière Electrique*, Vol. 22, No. 43, Oct. 23, 1886, pp. 161-60.)

The object of this article is to pass in review apparatus for multiple transmission in which currents from different sources or of different character, as, for instance, battery currents and induction currents, operate apparatus which are individually sensitive to one or the other kind of current.

In an English patent, No. 12,959, of the 7th February, 1850, Highton points out the possibility of using ordinary battery currents for working the instruments, whilst currents from electro-magnetic generators might be used for actuating sounders.

Werner Siemens, in 1856, proposed the simultaneous sending of ordinary battery currents, and of alternate currents produced by a magneto-inductor, over the same wire. The ordinary currents would act on any electro-magnets in the circuits, but these latter would be unaffected by the alternate currents, which could, however, act on an electro-dynamometer used as a relay.

Isham Baggs, in 1858, took out a patent for an arrangement for triple working. He used a magneto-electric current for deflecting a needle, a very powerful current which could have a chemical action on prepared paper, and very high tension currents which could perforate holes in paper strips.

The patent of Hughes, taken out in 1858, is also for the combined use of battery currents and of induced currents.

In 1871, Whitehouse proposed to make use of earth currents in conjunction with battery currents, each actuating its special instrument. As he puts it, the earth currents can be compared to the deep waves of the sea, slow and gradual in their motion, while the battery currents resemble ripples on the surface.

Varley's patent, No. 1,044, of 1870, is the forerunner of a great number of subsequent apparatus. He made use of rapidly undulating currents superposed on the ordinary currents, the former actuating special harmonic sounders.

In 1876, Elisha Gray patented a combination of harmonic telegraphs with ordinary Morse instruments, which, in 1878, was modified by the introduction of telephones in the place of harmonic telegraphs.

It is to the patent, No. 1,477, of 1879, taken out by Messrs. Black and Rosebrugh, that we have to look for the first arrangement of two parallel telegraphic wires as a telephonic circuit. The telephones are interposed between the earth and a condenser, which stops the battery currents while it allows the passage of the undulatory telephone currents, and of alternate currents for ringing up.

In this same year, 1879, we have Edison's American patent, No. 217,781, in which an arrangement is described capable of producing three variations of current. There is a key for reversing the current, a second for making

variations in the strength of the current, and a vibrating reed for producing an undulatory current.

Field's American patent, No. 242,411, of 1881, treats of a sextuple telegraph; two keys vary the intensity of the current, and a third produces harmonic vibrations; the receiving instruments are arranged to answer to each kind of current.

Coming now to more modern systems, we have Van Hysseberghe's plan of combined telegraphy and telephony; similar plans by Maiche and Van der Weyde; a combination of the telegraph with the phonophore by Langdon Davies; and two duplex telegraphic systems, one by Edison and the other by Rowbrugh. These systems are somewhat complicated and difficult to explain without the aid of the diagrams given in the article.

### M. DEPREZ—INTENSITY OF THE MAGNETIC FIELD IN DYNAMOS.

(*La Lumière Electrique*, Vol. 22, No. 45, Nov. 6, 1886, pp. 269-71.)

The most important part of a dynamo is the magnetic field. The magnetic field contains two elements, the volume and the intensity, which in different proportions constitute its useful qualities. If we have given the dimensions of the electro-magnets, the current in them, and the distance from the iron of the electro-magnet to the iron of the helix, the volume of the field can be easily calculated; but this is not the case with the intensity.

The intensity of the magnetic field was deduced from measurements of the force exerted upon a movable conductor in a direction at right angles to the lines of force. Contrary to what has been sometimes stated, the intensity of the field decreases much less rapidly than the distance apart of the pole-pieces increases. In one experiment, when the distance had been increased tenfold, the intensity of the field had only diminished one-half. For a single magnet with its armature, when the distance was quadrupled, the intensity decreased from 1 to 0.6.

In all cases, the larger the cores of the electro-magnets the greater the current which circulates round them, and the thicker the armatures the less the intensity of the field is affected by the distance between the armature and the magnet. It follows that, by bringing the armatures very close to the pole-pieces of the electro-magnets, the intensity of the field is only very slightly augmented, while the space for the wire on the armature is very considerably diminished. The efficiency of the dynamo is a maximum when the internal resistance is a minimum. Let  $x$  be the distance of the inter-iron space in a dynamo,  $a$  the minimum of the space occupied by the insulation of the wire, &c., and  $H$  the intensity of the field; then the internal resistance is a minimum when the expression  $H^2 (x - a)$  is a maximum.

With respect to the effect of the dimensions perpendicular to the lines of force, the author has noticed that, unless the pole-pieces are very thick in a direction parallel to the lines of force, the intensity of the field is inversely proportional to the total surface of pole-pieces reckoned at right angles to the lines of force.

**ANON.—DUPLEX TELEPHONY.***(L'Electricien, Vol. 10, No. 189, Nov. 27, 1896, pp. 771-73.)*

The system invented by Mr. J. Barrett, of Brooklyn, is in use on the lines between New York and Philadelphia, and permits of four instruments being used on two wires, with the restriction that only the two extreme posts can communicate together, or the two intermediate posts, but not an extreme post at one end with an intermediate one at the other.

The two intermediate posts, A and B, are identical, and contain each a battery, a microphone, and the primary of an induction coil; the secondary wire at each post is wound on two bobbins (*a*, *b*), and has in its circuit a receiving telephone. The two line wires are also wound on the same bobbins as the secondary of the induction coil. A post C is now connected by means of one end of its secondary wire to a point on the double wire between the bobbins *a* and *b* of the post A, the other end of the secondary circuit, which contains the telephone, being to earth; a similar arrangement for a post D is made in connection with B. If now C talks to D, *i.e.*, one extreme post to the other, the current on reaching the double wire divides, and if the bobbins *a* and *b* are suitably arranged the two actions will be equal; hence the intermediate posts A and B will not be affected. When A and B, the intermediate posts, talk together, the current follows only the line wire, and will not traverse the induction coils on account of their large coefficient of self-induction.

**ANON.—ELECTRICITY AND ATMOSPHERIC PRESSURE.***(Bulletin de la Société Internationale des Electriciens, Vol. 3, No. 81, p. 348.)*

A curious phenomenon has been observed by Mr. W. Hempel in connection with the working of a statical induction machine. It is that the quantity of electricity produced increases considerably in an atmosphere subjected to pressure. Thus a machine which would give 15 sparks per minute when revolving at a speed of 400 turns a minute, under the ordinary atmospheric pressure, could be made to give 32 sparks per minute when the pressure was doubled. If the pressure is still further increased, the quantity of electricity developed increases considerably.

**Dr. BOUDET DE PARIS—A NEW METHOD OF PRINTING BY ELECTRICITY.***(Bulletin de la Société Internationale des Electriciens, Vol. 3, No. 32, pp. 372-76, and Vol. 4, No. 34, pp. 24-8.)*

The object to be copied—a medal, a seal, or any other engraved or stamped object—is covered with a thin coating of graphite, and is connected to one electrode of a statical machine. The object is then placed, with the prepared side downwards, on a piece of cardboard, wood, silk, or other material on which the image is to be reproduced, and underneath which is a sheet of glass or

other dielectric laid above a metal plate or a sheet of tinfoil in connection with the second electrode of the machine. On working the machine, the graphite is carried from the prepared object on to the lower surface, and an exact reproduction of the design is formed. Whatever may be the method of engraving, in relief or intaglio, the image is always formed by the reproduction of the hollow lines, and not by the lines in relief; moreover, it is quite immaterial whether the positive or negative electrode is connected to the object to be copied. This is in contradiction to the generally received opinion, that the mechanical transference by the electric discharge is always in the direction from the positive to the negative electrode. It is even possible to copy a chalk drawing by this process on the top of the metal plate in connection with the one electrode is placed a sheet of glass, on this a sheet of clean paper, then the drawing, face downwards, and, finally, another metal plate in connection with the other electrode. After five or six discharges enough chalk was carried over to reproduce the drawing, while the original seemed as bright as ever.

Several theories have been advanced in explanation of this method of printing in a condenser, as it may be described; probably that of Mr. de Meritens is the true one. He thinks that the phenomenon may be explained by the ordinary laws of electrostatics. During the charging of the condenser all parts of the object are naturally at the same potential, but the electrostatic charge is greatest at the level of those parts which are farthest from the dielectric. Consequently, at the instant of discharging, the graphite is projected more violently on to those points which correspond to the hollows of the original object.

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#### **A. DE MERITENS—USE OF ELECTRICITY FOR RENDERING IRON RUSTLESS.**

(*Bulletin de la Société Internationale des Electriciens*, Vol. 3, No. 39, pp. 230-34; No. 33, pp. 413-14.)

The object to be attained is to cover the surface of the iron or steel with a very thin skin of magnetic oxide of iron, which then prevents all further action of the air. This is the so-called bronzing of gun barrels. The process as usually carried out is a very tedious one, the object being first allowed to take on a thin layer of rust, which is then scratch-brushed and burnished—the process being repeated several times over. Mr. de Meritens has found that by placing the objects to be operated upon in a bath of water, making them the anodes, and passing a current through the bath, they are covered in an hour or two with a layer of the magnetic oxide. By regulating the current so that the E.M.F. is just sufficient to decompose the water the layer of magnetic oxide is very dense, and adheres perfectly to the steel. When malleable iron was treated in the same way, the skin of magnetic oxide would not adhere; by first making the iron the anode a deposit of oxide of iron is formed; the connections are then reversed so that the iron is the



kathode, when the hydrogen evolved reduces the oxide; reversing once more, the layer of magnetic oxide produced was found to be perfectly adherent.

The first action of the decomposition of the water is the production, not of the magnetic oxide  $\text{Fe}_3\text{O}_4$ , but of the protoxide  $\text{FeO}$ , which is very little known, owing to the avidity with which it combines with a further quantity of oxygen to form the sesquioxide  $\text{Fe}_2\text{O}_3$ . If the iron covered with the layer of protoxide, produced in the first instance by the decomposition of the water, is at once removed from the bath and plunged into a solution of any metallic salt, it is covered with a film of the metal, copper, silver, gold, aluminium, &c., which is perfectly adherent. It is thus possible to coat iron with a skin of almost any metal.

### J. ZACHARIAS—CENTRAL ELECTRIC LIGHT STATIONS AT BERLIN.

(*Centralblatt für Elektrotechnik*, Vol. 8, No. 26, 1886, pp. 552-55; and No. 27, pp. 574-79.)

At the present time there are seven central stations in Berlin at work, though they are not all supplying the maximum number of lights for which they are intended. In the Schadowstrasse station are two dynamos working 500 glow lamps and 6 arc lamps; in the Friedrichstrasse, four dynamos supplying 1,800 glow lamps and 6 arc lamps; in the Markgrafenstrasse are fifteen dynamos capable of supplying 8,000 glow lamps and 26 arc lamps; in the Mauerstrasse, seven dynamos for 6,000 glow lamps and 36 arc lamps. These four stations are worked by the German Edison Company. The Berlin Lighting Company has a station with 7 dynamos supplying 900 glow lamps and 100 arc lamps in the Ausstellungs Park, and another with 8 dynamos for 4,000 glow lamps and 38 arc lamps in the Bauthstrasse. The seventh station belongs to Messrs. Siemens & Halske, in the Kaisergallerie, and has 3 dynamos for 3,000 glow lamps and 40 arc lamps.

In the Mauerstrasse station are at present three water-tube boilers and three compound steam engines, each of 150-170 actual horse-power. Each engine drives direct two Edison dynamos, made by Siemens & Halske, for 500 lights each, and also a counter shaft, from which are driven four Siemens dynamos, each for 12 arc lights. Each dynamo is connected to the main switch board by three cables, two for the main current and the third for connection to the resistances to be inserted into the exciting circuit. All the dynamos send their current into a common lead, built up of 12 copper strips, of about 50 mm. total area, which is connected to the 20 distributing mains. By means of suitable commutators the difference of potential at the terminals of each dynamo, as well as at various points in the circuit, can be measured on the same voltmeter. Each main conductor is fitted with a double-pole switch and a safety fuze.

The station in the Kaisergallerie has two boilers, three 80 horse-power engines, and three Siemens dynamos, each for 500 ampères and 65 volts. The wires are partly insulated in the cellars, and partly bare wires on insulators for the upper parts of the building.

The Markgrafenstrasse station has six engines, each driving two Edison



500-light dynamos. The distribution is made on the three-wire system by means of Siemens & Halske's lead-covered cables. These cables contain, besides the main conductor, a small insulated wire, by means of which the E.M.F. at various points in the network can be determined directly from the machine-room. There are the usual switches and measuring instruments for controlling the working.

**Dr. OTTO FEUERLEIN—ERHARD'S CIRCULATING BATTERY.**

(*Centralblatt für Elektrotechnik*, Vol. 8, No. 30, 1886, pp. 643-51.)

A number of rectangular plates of zinc are covered on one side with lead-foil and on the other with a cloth, which acts as a diaphragm. The battery is built up of these compound plates alternately, with three-sided frames of papier-maché, the same size as the plates, the open side being at the top, so that a space is left between each two plates to contain the solution. The two ends are closed by boards; the whole being held together by bolts passing right through. Above the battery is placed a glass vessel containing crystals of copper sulphate and some water. This vessel can be closed at the top, and is provided with a tubulure below, into which are fitted two tubes, one above the other, each of which leads to a distributing pipe with spouts opening into each cell. The battery being filled with water, the cocks on the two tubes are opened, the heavy solution of copper sulphate flows out of the vessel by one tube into the cells, and the water rises through the other tube and dissolves a fresh quantity of the sulphate. The lead plates are at once covered with a layer of metallic copper and the battery then works like any ordinary Daniell's cell. As the zinc is dissolved the specific gravity of the ascending liquid becomes gradually heavier, and, finally, the circulation ceases and the battery has to be re-charged.

The largest size of battery consists of 17 cells, and has a useful surface of 450.5 sq. cm. One of these was submitted to ten discharges of from 3 to 8 ampères, and a total output of 305 ampère hours. The E.M.F. was on the average about 15 or 16 volts; the internal resistance for the greater part of each discharge was less than 1 ohm, but rose at the end of each discharge to about 1.5 ohm. In the total of 70 hours' work 8.8 kilos of zinc were dissolved, and 40 kilos of copper sulphate used up; these quantities are in excess of those required theoretically, viz., 6.3 kilos of zinc and 24 kilos of copper sulphate. The experiments on the two smaller sizes showed analogous results, but not so satisfactory as those obtained with the larger size.

**Dr. J. KLEMENCIC—THE RATIO OF THE ELECTROSTATIC AND ELECTRO-MAGNETIC SYSTEMS OF UNITS.**

(*Centralblatt für Elektrotechnik*, Vol. 8, 1886, No. 30, pp. 661-66; and No. 31, pp. 689-98.)

The capacity of an air condenser is first determined accurately in electrostatic units; its capacity in electro-magnetic units is then measured by

charging it to a known potential, allowing it to discharge for a known time through a high resistance, and then determining the potential of the residual charge.

If  $q$  and  $Q$  are the quantities of electricity corresponding to the potentials after and before the discharge, then

$$q = Q e^{-\frac{t}{CR}}$$

Taking into account the self-induction  $S$  of the discharging wire, and developing the above formula, we get finally

$$q = Q e^{-\frac{t}{CR}} \left( 1 + \frac{S}{CR^2} \right).$$

In the experiments, the condenser was charged and discharged by means of a tuning-fork carrying platinum points dipping into mercury cups. The discharge to earth took place through a column of zinc sulphate solution having a resistance of 1 megohm, a resistance box, which could be varied within wide limits, and a shunted galvanometer.

If  $D$  is the deflection for full discharge, and  $d$  for partial discharge, then we may write the above equation

$$D - d = D e^{-\frac{t}{CR}} \left( 1 + \frac{S}{CR^2} \right),$$

$$\log. \left\{ \frac{D \left( 1 + \frac{S}{CR^2} \right)}{D - d} \right\} = C_0 R_m$$

Hence 
$$t = \frac{C_0 R_m}{\log. e}.$$

where  $C_0$  is the capacity in electrostatic, and  $R_m$  the resistance in electromagnetic units. The correction factor  $\frac{S}{CR^2}$  amounted to only 0.0037. Particular attention was paid to the mercury contacts, on the action of which Lord Rayleigh has thrown some doubt; but they were found to work perfectly when due care was taken to keep them in proper order.

Full details of the several parts of the apparatus are given, as well as particulars of each of the several measurements to be made; but it is only possible to note the final result obtained as a mean of the values given by numerous experiments, viz.,  $v = 3.015 \times 10^{10}$ .

# **A. PARES—HYDROPHONE, OR MICROPHONIC APPARATUS FOR TESTING LEAKS IN WATER PIPES.**

(*Centralblatt für Elektrotechnik*, Vol. 8, 1886, No. 34, pp. 776-78.)

The sounding rod, the lower end of which is in contact with the water-pipe, is supported vertically in a portable tripod stand. The microphone screws on to the top of the sounding rod, and is provided with two sets of terminals; to one pair are attached the wires connecting the telephone, and to the other pair the wires from the battery. In one of these wires is introduced a pear push, such as is used for electric bells. The inspector, holding the

telephone in one hand and the push in the other, can then put on the current when necessary, and ascertain if the telephone gives any sound. The microphone has also a ring at the top by means of which it can be lowered down into the pipes or other receptacles to be tested. The whole apparatus packs away into a box, and is quite portable.

### **Dr. V. WIETLISBACH—LONG-DISTANCE TELEPHONY.**

(*Zeitschrift für Elektrotechnik*, Vol. 4, Pt. 10, Oct., 1886, pp. 463-69.)

All technical difficulties in the erection of long telephone lines are now removed, and the whole question is one of economy. The results obtained by Van Rysselberghe, in America, have shown that conversations over long lengths of wire are quite feasible (see *Journal*, vol. xv., pp. 291 and 376).

The essential point in long-distance telephony is the conductor; the apparatus at either end is of secondary importance. Lord Rayleigh has applied Maxwell's theory to Hughes's practical results on induction, and he shows that for cylindrical wires the change in resistance for a variable current is proportional to the square of the number of alternations and to the fourth power of the diameter of the wire. It is also proportional to the constant of magnetisation, which is 1 for copper and about 300 for iron. For copper wires of the sizes used in practice the variation from Ohm's law is not great, but for iron wires it is considerable; thus, with an iron wire 1 mm. in diameter, the resistance is increased about  $\frac{1}{3}$  for 1,000 vibrations; with 2 mm. wire, for the same number of vibrations, the resistance is doubled; with a 4 mm. wire the resistance is doubled for 300 vibrations, and becomes from 5 to 10 times greater for 1,000 vibrations.

The dependence of the resistance of the line on the number of vibrations of a current in the variable phase is particularly prejudicial for long-distance telephony, because the various notes of the human voice each correspond to a different number of vibrations, and hence the resistance of the line is less for the deep tones with a less number of vibrations than for the high notes with a greater number. For instance, on a 4 mm. iron wire the resistance will be increased 50 per cent. for the note G of 200 vibrations, whilst for its octave, G<sup>1</sup> of 400 vibrations, the resistance will be increased 100 to 150 per cent. The deep notes, therefore, come out relatively too loud, and the timbre of the voice is altered. This has been proved experimentally, both by the author and by Preece. It is quite clear, therefore, that for long distances, say beyond 200 kilomètres, no arrangement of transmitting and receiving apparatus will overcome the difficulties of communication due to the increased resistance of iron wires for currents in the variable phase, but the line must be built of copper wire.

It is also necessary to avoid disturbances from outside, either arising from earth currents or from currents in neighbouring wires; for this purpose the circuit should be metallic throughout, or, in other words, the line must be looped; and the insulation should be as perfect as possible, as any leakages introduce disturbances. Where several wires are run on the same posts, they

should be arranged alternately on the arms, so as to cross each other, and not be parallel for any distance.

In order to avoid the expense of running a second wire to each subscriber, it has been proposed to use translators, or induction coils, consisting of an iron core on which are wound two coils of wire, one coil being connected to the main looped line and the other to the local wire. This arrangement has not given good results in practice, chiefly owing to the magnetic inertia of the iron cores in the coils, and it is preferable to adopt an arrangement devised by Bliss, in which the return wire is common to several subscribers. Normally only the single wire is connected up for calling the subscriber, but when he wants to talk the return wire is switched on to the particular instrument in use.

Experience shows that the increased cost of looped lines is made up for by the increased facilities of communication; besides, owing to the increased facility of working, more messages can be sent over the wire in a given time.

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# JOURNAL

OF THE

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Telegraph-Engineers and Electricians.

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The One Hundred and Sixty-fourth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, March 10th, 1887—Sir CHARLES T. BRIGHT, M.Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from Mr. J. Aylmer, Local Honorary Secretary for France, and Sir David Salomons, Member. The SECRETARY also announced that a series of portrait engravings of eminent electricians had been received from Mr. C. H. W. Biggs, Member.

A hearty vote of thanks was accorded to the donors for their presentations.

The following paper was then read :—

## ON REVERSIBLE LEAD BATTERIES AND THEIR USE FOR ELECTRIC LIGHTING.

By DESMOND G. FITZ-GERALD, Member.

When the cheap production of electrical power by means of the dynamo machine was first realised as a *fait accompli*, there were many who naturally, but somewhat rashly, assumed that cheap electrical power would immediately become generally available for electric lighting in private dwellings and for many other applications on a small scale. These anticipations, we all know, were doomed to disappointment—most of us have found it impracticable to introduce electrical power into the household; and the privilege of making use of the most beautiful, cleanly, and hygienic means of artificial lighting has hitherto been denied to us. It may be interesting at the present moment—when the lead storage battery is claiming renewed attention—to glance briefly at the grounds for the sanguine anticipations which were formed soon after Werdermann brought the machine of Gramme into this country, to recall some of the main causes of their non-realisation, to examine critically the accepted views as to the chemical and electro-chemical reactions involved in the working of reversible lead batteries, to estimate the probable extent to which such batteries may be improved, or the room there may be for improvement, and, lastly, to consider, at least in its *prima facie* aspect, the question as to the practicability of carrying the original anticipations into effect by means of an improved form of reversible lead battery.

It is admitted by high engineering authority that, with steam engines of improved construction and of not less than 300 h.p. nominal, it is quite practicable to obtain a h.p.-hour mechanical by the combustion of two pounds avoirdupois of good coal. Allowing 20s. per ton for such coal, the cost of the fuel to develop this quantity of energy would be considerably under one farthing (·214d.). Putting the commercial efficiency of the dynamo machine at the very low figure of 70 per cent., the



cost of the h.p.-hour electrical would be less than one-third of a penny (.306d.). Taking everything into account, Sir W. Siemens in 1882 could not estimate the cost of production, in London, at a higher figure than nine-tenths of a penny (.895d.). There are incandescence lamps that will yield a candle-power for every  $2\frac{1}{2}$  watts expended; and, taking the h.p. electrical as 736 watts, it would thus be equivalent to the illuminating power of 294 standard candles. The price, in London, of the gas to produce this illumination for one hour may be estimated at about  $2\frac{1}{2}$ d.; and in London gas is cheaper than in most other localities. It was natural that figures analogous to these should arouse sanguine anticipations in the electrical engineer—

And duller should we be than the fat weed  
That rots itself in ease on Lethe's wharf  
Were we not stirred by this.

But when, in this country and elsewhere, the requirements for domestic electric lighting came to be practically considered, it was found on the one hand that—in spite of the capabilities of the individual celebrated amongst our Parisian *confrères* as *le jarlinier de M. Preece*—no private individual would, as a rule, willingly incur the prime outlay, trouble, and comparative expense of a small steam or gas engine and a dynamo machine; and, on the other hand, that the cost and difficulty of laying down leads to supply even a populous district with electrical power from a central station was a very serious consideration. After 1882, when electric lighting was made a stalking-horse by the worst sort of company-promoter, it was clearly seen that this mode of supply, involving an investment of capital even larger than that which had gradually been made in gas, would be out of the question for many years to come. I have no doubt that it will ultimately be adopted to a large extent—but this will not be, I think, in the lifetime of those of us who have passed the middle-age; nor do I think that the distribution of electrical power from central station will at any period altogether supersede the method of distribution, by means of elements in which electrical energy has been stored, to which I shall presently advert.

The electro-chemical means at present available for the

storage of electrical energy cannot, either from a scientific or a practical point of view, be regarded as perfect; and it seems certain that they will be considerably improved in future years. The trite saying that the storage battery is still in its infancy is perhaps not inapplicable; but it would be a mistake to ignore the fact that important improvements have been made in this apparatus since (seven years ago) the cell of Gaston Planté was first modified by Camille Faure. Under the headings of "Storage Capacity" and "Weight per Horse-power-hour," I have jotted down from my note-book, in tabular form, some figures, subject to correction and amplification, which in some measure illustrate this improvement, although there are other points of equal importance which require to be taken into account. The "lithanode" mentioned in these tables was the subject of a paper which I read at the British Association Meeting at Birmingham, and which will be found in *The Electrician* of September 10, last year. It is peroxide of lead (with more or less sulphate of the metal) in a coherent and highly-conductive form, having generally a specific gravity between 7.5 and 7.9. I shall have occasion again to refer to this material presently.

Table I.

## STORAGE CAPACITY OF VARIOUS SECONDARY CELLS.

Name of Cell.	Per lb. of Pb.		Per kilo. of Pb.		Authority.
	Foot lbs.	Watt hours.	Kilogram-metres.	Watt hours.	
Planté ... ..	12,000	4.52	3,664	10	Howard.
Faure .. ..	18,000	6.78	5,495	15	
E.P.B. L plates ...	48,000 (?)	18.09 (?)	14,600 (?)	39.8 (?)	
" B " ..	36,080	13.6	11,010	30	(?) Hospitalier.
" S nominal } 22 lb. cell ... }	31,800	12	9,540	26	Fitz-Gerald.
Elwell-Parker (old form) ... }	6,633	2.5	2,018	5.5	Prospectus.
Lithanode battery } (old form) ... }	39,798	15	13,110	33	Fitz-Gerald.
Lithanode battery } "Union" cell }	47,170	17.8	14,671	39.16	G. Forbes.

Table II.

WEIGHT PER HORSE-POWER-HOUR CAPACITY OF VARIOUS  
SECONDARY BATTERIES.

Name of Battery.	Elements only.		Cell complete.		Authority.
	Lbs.	Kilos.	Lbs.	Kilos.	
Planté ... ..	...	...	396	180	Reynier.
Faure ... ..	...	...	88	40	Faure.
	...	...	165	75	Sir W. Thomson.
" (old model) ..	...	...	198	90	} Reynier.
" (new model) ...	...	...	184	81	
E.P.S. L plates ...	...	...	133	60·4	Prospectus.
	...	...	110	50	Reckenzau.
" 8 " ... ..	66	30	100	61·3	Fitz-Gerald.
Reynier { Zinc posve.	50·8	23	117·5	53·4	R. Tamme.
	Plantéform	105	47·6	...	<i>Idem.</i>
Lithanode battery	42	19·1	76	34·5	Fitz-Gerald.
(old form) ... }					
Lithanode battery	42	19	70	31·5	G. Forbes.
" Union " cell }					

In Table I. I have taken the liberty of placing a note of interrogation after the values, relating to the storage capacity of the plates formerly manufactured by the Electrical Power Storage Company, taken from the paper read by my friend and old pupil, Mr. F. G. Howard, in June, 1885, at a meeting of students at the Institute of Civil Engineers. No doubt Mr. Howard referred to the total storage capacity, instead of the useful capacity, of these plates; but even in this case the value given would seem to be much too high. Certainly the plates now issued by the Electrical Power Storage Company are at least equal to those manufactured by them two years ago; and at the present moment the useful storage capacity claimed by them per lb. of battery is, I believe, 3 ampère-hours on a rough average. In the smaller cells this value may be surpassed—in the larger it is scarcely attained. The present "thin-plate" Electrical Power Storage cells have a capacity of 2·8 ampère-hours per lb. of battery. In practice, as for instance at the

Colonial and Indian Exhibition, this value, we know, is not always even approached. Now, taking the high value of 3 ampère-hours per lb. of battery, the h.p.-hour cell would weigh 129 lb. But, according to the values I have called into question, 41 lb. of lead in the battery would suffice to produce the h.p.-hour; and this lead would constitute only one-third of the total weight of the battery—a ratio which is far too small, and requires to be increased to at least one-half.

A high ratio of stored energy to weight is not always the most important consideration in a reversible lead battery; and the few data I have collected in tabular form would have been of greater interest if I had specified in each case the *rate of discharge*, since it is well known that the useful capacity becomes diminished as this value is increased. I will to some extent remedy the omission by stating, in regard to the lithanode battery, that when the rate of discharge is about  $\cdot 446$  ampère per lb. of plates, or  $\cdot 257$  ampère per lb. of battery, the useful capacity may be as high as 9.32 ampère-hours per lb. of plates, or 5.3 ampère-hours per lb. of battery. But when the rate of discharge reaches  $\cdot 64$  ampère per lb. of plates, or  $\cdot 36$  ampère per lb. of battery, then the useful capacity falls to 8.6 ampère-hours per lb. of plates, or 4.8 ampère-hours per lb. of battery. These results have already been far surpassed in the laboratory; but they are probably the maxima hitherto obtained on a practical scale. I have just been informed, however, that the figures I have given are not considered applicable to the "Union" cell, and that the results obtained in recent experiments are expressed by the following table:—

UNION CELL.			
Rate of Discharge.		Capacity.	
Ampères per lb. of		Ampere-Hours per lb.	
Plate.	Cell.	Plate.	Cell.
$\cdot 68$	$\cdot 41$	9.2	5.5
$\cdot 88$	$\cdot 53$	7.1	4.3
1.2	$\cdot 72$	6.3	3.7
1.5	$\cdot 9$	5.5	3.3

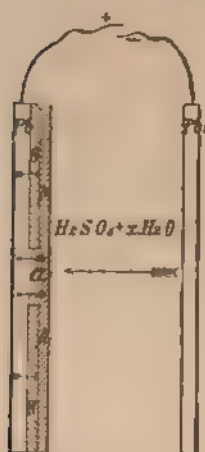
Whatever may be said of the lead storage battery itself, it is quite certain, I think, that our electro-chemical knowledge in relation to it is "in its infancy." We should be thankful even for small mercies in this direction, for very few electricians or electro-chemists have given any real attention to the matter, which is one of vital importance to progress. But we should be specially grateful to Dr. J. H. Gladstone and to the late Mr Alfred Tribe, who gave a great deal of careful labour to "The Chemistry of the Secondary Batteries of Planté and Faure."\* I am glad to say this before criticising, to the best of my ability, some of their conclusions from which I venture to differ—or to imagine that I differ.

In the first place there is a little matter—perhaps a *lapse*—of no great importance possibly in itself, but which seems to me of importance by reason of the detestation in which I hold anything in the form of "white sulphate"—non-conducting sulphate—in the negative element of a lead storage cell. In their little work, under the above-mentioned title, Messrs Gladstone and Tribe propound the following question and answer: "In a Planté or Faure battery, the mass of peroxide which is in contact with the (negative) metallic lead plate expends its energy slowly. How comes it to pass that if the same mass of peroxide be brought into connection, through the first lead plate, with another lead plate at a distance (in the same electrolyte) it expends its energy, through the greater length of sulphuric acid, in a tenth or a hundredth part of the time? The answer . . . is doubtless to be found in the formation of the insoluble sulphate of lead, which clogs up the interstices of the peroxide, and, after a while, forms an almost impermeable coating of high resistance between it and the first metallic plate." The following diagram, I take it, represents in an exaggerated and conventional manner the conditions here indicated; Pb and Ph, being the two lead plates, *p* the layer of peroxide, and *s* the almost impermeable coating of sulphate of lead. *a* is the point where the peroxide *p* is in contact with the lead plate Pb. Now it appears to me that this suggested explanation involves a misconception as to the conditions which

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\* Their work, under this title, is published by Macmillan.

are favourable to the passage of the local current, of which the direction, as shown by the small arrows, is from the metallic



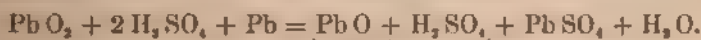
support to the peroxide through the electrolyte, and from the peroxide to the metallic support by simple conductive contact. Not that the resistance (electrolytic) between the supporting plate and its peroxide is *common* both to the local circuit and to the main circuit; for there must be a simple conductive contact, as at *a* in the diagram, between the plate and the peroxide at one or more points (otherwise the detached layer of peroxide would become inactive, and there would be no current in either circuit), and the main current would pass wholly through this contact

—as, indeed, would the local current in its passage from the peroxide to the support. But it is evident that the local current, under the given electro-motive force, will be inversely as the sum of the resistances in its circuit, and that one of these resistances—that at *a*, which opposes its passage from the peroxide to the support—must be small (since otherwise the main current would be impeded). The remaining resistance in the local circuit—that which opposes the passage of the local current from the lead support to the peroxide *through the electrolyte*—must evidently be very considerable in order to comply with the conditions under which the current in the main circuit may be ten or a hundred times greater than that in the local circuit. Now, in my view, the electrolytic resistance in the local circuit would be diminished, instead of augmented, by interposing even an almost impermeable (but more or less moist) coating of sulphate of lead between the peroxide and its support, excepting at one or more points where the peroxide and the lead would be in perfect contact. To maintain perfect unbroken contact everywhere between the layer of peroxide and the lead supporting-plate would, I think, be the best means of diminishing local action, by augmenting the electrolytic resistance in the local circuit. And this, in substance,

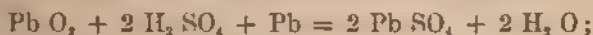


was the burden of the paper read by Messrs. Drake and Gorham at the British Association last year.\* The clogging up of the *interstices* in the peroxide might, I am ready to admit, be beneficial, provided this unbroken contact were maintained. It may be observed, however, that the suggested explanation appears to be further disproved by the figures given by the authors of the work referred to (p. 6). During the first two hours, when the quantity of sulphate formed is a minimum, the quantity of peroxide reduced by local action is only 3·6 per cent. per hour. In the next hour, when the quantity of sulphate must have augmented, the percentage of peroxide reduced is shown to be nearly 8; and in the following hour it rises to about 10. It is true that this percentage afterwards diminishes, but this may well be due to a diminution of the E.M.F. acting in the local circuit.

Passing to a matter of more general importance, the equation given by the writers I have named as applicable to the batteries of Planté and of Faure — and presumably also to all lead secondary batteries with the same electrolyte—is substantially as follows:—



The compounds bracketted together react, however, upon each other, so that the final result may be expressed by



the final result being sulphate of lead on both plates.

This equation is very simple, and has been generally accepted as entirely satisfactory. As an expression merely of the chemical results observed, after a certain percentage of the peroxide has become reduced, it is perhaps unassailable. As an electro-chemical equation, applicable during the whole period of the discharge of a lead secondary battery with the given electrolyte, it is impossible, I think, to accept it. It is not the old fallacy—recently protested against by Mr. Swinburne, and which is to be found in this equation—of supposing that water, and not an acid or a salt, is the electrolyte in the voltaic battery, that I have now

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\* "On the Treatment of Secondary Batteries."



to dwell upon. I will endeavour to lay before you the other difficulties I have found in the way of its acceptance.

But in the first place it will be expedient to consider whether there has been any intention of putting forward or accepting the above formula as an electro-chemical equation expressing the reactions on which the efficiency of the battery is dependent. Has there actually been any assumption that the whole of the peroxide is, or may be, converted into sulphate of lead? that a large proportion of isolated sulphate of lead "white sulphate"—is necessarily produced in discharging the battery? that the reduction of the peroxide takes place, not in two or more stages, but continuously? and that the electro-motive force of the battery is independent of the quantity of sulphate which has been produced from the peroxide? Because, if not, I might be setting up imaginary antagonists and incurring unnecessary trouble in endeavouring to overcome them.

The authors of the work already referred to make use of the expression given above: "The final result being sulphate of lead in both plates." Dr. Gladstone, moreover, has explained that "When we stated that sulphate of lead is finally the only product of the discharge, we were referring to the disappearance of any peroxide, and did not mean to imply that in actual practice the whole of the spongy lead is usually converted into sulphate." Mr. Tribe, in *The Electrician* of July 10, 1885, wrote that "One of the conclusions was that both the peroxide of the negative plate and the finely-divided lead of the positive were converted into lead sulphate during discharge." Mr. George F. Barker, in his paper read at the Montreal meeting of the American Association for the Advancement of Science, says: "My experiments with the Faure battery confirm entirely those of Gladstone and Tribe as to the formation of lead sulphate." "On examining the plates, lead sulphate formed the entire coating upon both of them." Mr. C. T. Kingzett states that "As fast as the monoxide of lead, produced by reduction at the negative pole during discharge, forms, it is converted into sulphate. Upon complete discharge then there remain two supports of lead coated with sulphate of lead." And Professor Oliver Lodge says: "The use

of peroxide alone (without any support of lead) looks hopeful; but, when the cell is discharged and the peroxide reduced, the plate will no longer be a conductor—and it does not appear probable that such a cell could ever be charged up again." This conclusion, I may here state, is emphatically disproved by the "lithanode" battery plates now before you, most of which have been charged and discharged—down to the potential of 1·8 volt—a great number of times, without losing their conductivity or exhibiting the smallest patch of "white sulphate."

Mr. G. F. Barker further observes that, "Obviously, so long as any peroxide is present, the electro-motive force is constant." The same experimenter also makes the following very suggestive note, showing that one at least of his cells had been, apparently accidentally, worked upon an improved system. He says: "In one of my Planté cells, the peroxide is beautifully crystalline and very hard. Not a trace of (free) sulphate has been formed in it, apparently, though it has been in use for six months, and has been frequently charged and discharged during that time."

On the other hand, Messrs. Gladstone and Tribe state (*loc. cit.* p. 32), in relation to the negative plate: "At the conclusion of the action, we have always found more or less of the substance unaltered. Thus, as one instance, after a discharge lasting five days, and approximately complete, we found that only 68 per cent. of the deposit was lead sulphate." And Mr. J. Swinburne (in *The Electrician* of July 10, 1885) makes an important observation in the same direction. He says: "The appearance of the coating of an ordinary discharged peroxide plate does not prove the absence of sulphate, for only a very small proportion can be sulphate. Some years ago, when I made a large number of experiments on batteries, I found that in no case more than six or seven per cent. of the coating is used, even when the cell is completely run down. The expansion of the peroxide of lead in becoming sulphate perhaps blocks the coating up."

Let me at once observe that if Mr. Barker were right in stating that the E.M.F. is constant so long as any peroxide is present I should have less difficulty in accepting the above-mentioned formula as the electro-chemical equation applicable

to lead secondary batteries. But, like Mr. Swinburne, I have found that long before the whole of the peroxide is exhausted the E.M.F. has fallen practically to zero. If the formula be true, as the electro-chemical equation of the battery, why should there be any fall in the electro-motive force before the peroxide is exhausted? Is it because the peroxide becomes "blocked up" or clogged, or so diluted by the inert material, that the support becomes rapidly "polarised" by hydrogen? If so, the formula may be true. But on open circuit, when there can be no polarisation, and with a strip of solid peroxide without support, we should, if the formula be true, obtain the full E.M.F. corresponding to the nature of the chemical reaction indicated by it. We should expect this so long as any peroxide remained—certainly when only 25 per cent. of the quantity originally present had been consumed.

For a true "battery equation" is of course independent of the resistance of the circuit, and indicates a definite E.M.F. Conversely, if the E.M.F. alters—temperature and certain other conditions remaining the same—we know that the given battery equation no longer applies, that the original chemical reaction has given place to or has become complicated by another chemical reaction.

I take two strips of platinum, to be used as negative elements with a positive of spongy lead in an electrolyte of dilute sulphuric acid. One platinum strip (A) I coat with a paste of electrolytic peroxide of lead; the second strip (B) I coat with a paste composed of one part of electrolytic peroxide of lead and one and one-third part of sulphate of lead (equivalent weights, roughly), mixed together and with water. The couples are circuited, consecutively, through 60,000 ohms and a reflecting galvanometer of high resistance. The A couple, as soon as the negative becomes moistened by the electrolyte, gives a steady deflection of 130 divisions, corresponding to an E.M.F. of about two volts. I short-circuit this couple for 30 seconds, or for ten times this period—of course the deflection falls to zero; but, when the short-circuit is broken, the spot of light moves back to 130 within a few seconds. The B couple gives 128 divisions, falling steadily, within a few

minutes, to 72 divisions, corresponding to rather more than one volt (this through a resistance of over 60,000 ohms). I short-circuit this couple for one second: on breaking the short-circuit the deflection is 20 divisions, becoming in five minutes 67 divisions. A strip of plain platinum gave a deflection of 56 divisions, falling in five minutes to 42. It must be admitted that the result obtained with B was somewhat better than this; nevertheless, the effect of the sulphate in reducing the efficacy of the peroxide is sufficiently striking.

Still more so is its effect in the case of solid peroxide (lithanode) not in contact with any other simple conductor within the electrolyte. But in order to judge of the effect in this case, we must be able to analyse the lithanode chemically at the outset, and also when the E.M.F. between it and spongy lead has fallen to any given extent.

A convenient and, with due care, an accurate method of determining the percentage of lead peroxide in "lithanode," or any other active material containing this oxidant, is to add cautiously the finely-powdered material to the solution of a known weight of crystallised protosulphate of iron, acidulated with somewhat more than half an equivalent of hydrochloric acid, until the solution no longer gives a blue coloration with a drop of potassic ferricyanide solution. It is advisable to triturate the powder in a mortar containing the acidulated ferrous solution at a temperature over 100° Fah. Of course it is generally possible, especially after a rough preliminary trial, to add at once sufficient of the powder to nearly convert the whole of the ferrous salt into ferric salt; afterwards, the powder is added in minute portions until the blue coloration is no longer produced. If too much of the powder be added, free chlorine and a distinct brick-red coloration are observable. In regard to the numerical data, 7·8 grams of the ferrous salt ( $\text{Fe SO}_4 \cdot 7 \text{ H}_2\text{O}$ ) are equivalent to 1 gram of chlorine or to 3·36 grams of  $\text{Pb O}_2$ .

I took a fragment of one of my old lithanode plates (marked X), of which the capacity was about 16 ampère-hours (at a potential not lower than 1·8 volt), and found by the above method that it contained 70·7 per cent. of  $\text{Pb O}_2$ . I then made

use of another similar fragment of the plate as a negative element, until the difference of potentials between it and spongy lead fell to 1·8 volt. This fragment having been washed, and slowly dried at a low temperature, was reduced to powder and found to contain 55 per cent. of the  $\text{PbO}_2$  originally present. Only 22·2 per cent. of this quantity of peroxide had become reduced, 77·8 per cent. was apparently unaffected; and yet the reaction upon which the E.M.F. is dependent had become in some way modified, since the electrometric indication on open circuit had undergone a permanent diminution to the extent of about 10 per cent.

The weight of peroxide present in one pound avoirdupois of the original plate was  $\frac{16 \times 70.7}{100} = 11.3$  oz. In becoming exhausted to the extent of no longer giving with spongy lead an E.M.F. above 1·8 volt, one lb. of the plate will lose 2·5 oz. of peroxide. To what number of ampère-hours does this weight of peroxide correspond?

The ampère-hour equivalent for silver, according to Lord Rayleigh's latest determination, is 62 137 grains; this equivalent for peroxide of lead would therefore be

$$62.137 \times \frac{239 \text{ (i.e., mol. wt. of PbO}_2\text{)}}{216 \text{ (i.e., twice at. wt. of Ag.)}} = 68.75 \text{ grains.}$$

Now the number of grains in 2·5 oz. avoirdupois being  $2.5 \times 437.5 = 1094$ , the number of ampère-hours corresponding to 2·5 oz. of  $\text{PbO}_2$ , or to 1 lb. of the "X" lithanode will be

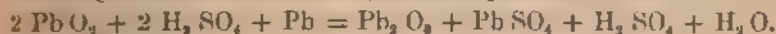
$$\frac{1094}{68.75} = 15.9.$$

It may be admitted, therefore, that when, as in the case of lithanode, local action in the negative element is entirely eliminated, the yield of the battery in ampère-hours is strictly proportionate—as might have been anticipated by theory—to the quantity of peroxide reduced in each cell.

This experimental result is interesting from other points of view than that of the electro-chemical equation of the lead storage battery. From the present standpoint of practice, 16 ampère-hours of useful capacity per pound of negative

element is a splendid result. With lithanode of more recent manufacture it has been surpassed; but it has never, I think, been approached with any other form of negative element. And yet, from the theoretical standpoint, what a poor result: two-and-a-half ounces of peroxide utilised in one pound of negative plate containing eleven-and-a-third ounces of the peroxide! It is not likely that we shall stop here: there is ample room for further improvement.

It might perhaps be expected that I should suggest a battery formula in substitution for that to which I have objected. I am sorry that I am unable to do so, nor even with any degree of confidence to attempt an explanation of the fact that so large a percentage of the peroxide present in a negative element should remain useless and inert for all practical purposes. I have thought it possible that the residual peroxide of lead might be in a state of combination, that a comparatively inert sesquioxide of lead might be formed according to the equation



But although this view, on the assumption that the sesquioxide formed may resist or be protected from the action of the sulphuric acid present in the electrolyte, may explain the recovery of apparently exhausted plates when they are left in contact with the acid, I have not been able to bring it into general accordance with the results of experiment and with those obtained by chemical analysis. It should be mentioned, however, that, although it is in opposition to the experimental results obtained by Gladstone and Tribe, this view is in some measure at least confirmed by those obtained by Shenck and Farbaky. These investigators found that, in charging a lead secondary cell, 2.23 grams (34.426 grains) of  $\text{H}_2 \text{ SO}_4$  become liberated per ampère-hour current; and that, in the discharge of the cell, 2.25 grams (34.736 grains) of the acid enter into combination. Now the ampère-hour equivalent of sulphuric acid is 1.826 grams (28.192 grains); and we may assume that at least this quantity enters into combination with the spongy lead positive in the production of the ampère-hour current. There would remain, therefore, only  $34.736 - 28.192 = 6.544$  grains of  $\text{H}_2$



$\text{SO}_4$ , or  $\cdot 23$  of the equivalent, to combine with monoxide of lead reduced from the peroxide in the negative element. The ampère-hour equivalent weight of oxygen would be derived from this element, an ampère-hour equivalent of lead monoxide would be produced; but only  $\cdot 23$  of the latter equivalent would become converted into sulphate, leaving  $\cdot 77$  of the equivalent in admixture or in combination with residual peroxide of lead.

Having had some experience of the behaviour of sulphate of lead when used as a depolariser, I have thought it possible also that this salt might undergo an initial effect of reduction; but this view, again, has not been confirmed by any experiments I have been able to make. What is required is discussion on these matters: this will probably provoke other suggestions which may lead to the extension of our knowledge.

I fear I have already trespassed too far upon your attention; but I have yet to submit to your consideration, as briefly as may be, a scheme for the supply to private householders, who may be desirous of adopting the electric light on a small scale, of the electrical energy necessary to realise their wish. This would be supplied from central stations (but without any outlay for insulated conductors beyond the "wiring" of the actual domiciles to the extent rendered necessary by the number and position of the lamps required) in the form of charged plates—the positives being conveyed in a closed receptacle and in a damp condition, and the negatives in a dry state. Sulphuric acid also would be required; the quantity per h.p.-hour electrical being, according to the accepted theory, 3.12 lb. of  $\text{H}_2\text{SO}_4$ . I think, however, that it will be found that considerably less than this quantity—say 2 lb. of the strongest commercial acid—will suffice. This acid would be supplied by the carboy; and a measured quantity would be added to each cell when the exhausted elements are replaced by fresh ones.

For each h.p.-hour electrical supplied—corresponding, we will say, to three 20-candle lamps maintained incandescent for five hours—we should have, in the stage which the practical solution of the problem has at present reached, to transport about 40 lb. of battery plates. It may be safely said that the



total weight to be transported will be under 45 lb. The maximum distance to which this weight would require carriage would probably be under half a mile—for it would be more economical to multiply stations than to augment this distance. On the return journey, the exhausted plates and their receptacles, practically the same in weight, would have to be conveyed.

One ton weight of plates distributed would correspond to at least 56 h.p.-hours electrical. The question is, What would be the cost, under conditions of practice which admit of considerable latitude, of distributing this weight of material? Upon the answer to this question depends the extent to which this system of distributing electrical energy could be commercially developed. A small percentage of the general public, if safeguarded from all trouble, would be willing to pay even as much as 1s. per h.p.-hour electrical. A very large percentage indeed, I am convinced, would pay 4d. for the same quantity of available electrical energy. Anywhere between these two limits a considerable amount of business might probably be done. Nothing but an actual trial of the system, on a small scale perhaps, but extending over a considerable period of time—so as to give some scope to the capacity for organisation, which has accomplished such marvels in our postal department—will give a conclusive answer to the above question. It is easy to suggest figures which would show the system to be absurdly impracticable; but, as I have elsewhere observed, it would be easy also on the same lines to show that the milkman, who, anywhere in his district, will send horse and cart, man and can, to deposit even a ha'p'orth of milk at the area door, must be financially in desperate case. But there may be some, here or elsewhere, who have had experience of work analogous to that which I have in contemplation; and their opinion, as well as the suggestions they might possibly offer, would be entitled to respect, and might be of considerable value.

In reading over what I have written I find I have said nothing on the subject of the spongy lead positive element of the lithanode battery. The reason is that the main difficulties to be overcome—the real points of importance—have been in the

negative element of the couple—often erroneously termed the positive element or plate, because it happens to be the positive electrode in charging the battery. To manufacture an efficient positive, from the lithanode unconverted material or otherwise, was comparatively an easy matter; and there is a choice of several positives which leave little or nothing to be desired. I have now only to say that there are a number of batteries at work under the table before you: there are 14 cells, each weighing about 20 lbs.; each cell has a charging capacity of  $\frac{7}{8}$  h.p.-hour, so that three and a half of them equal one electrical h.p.-hour. The two 5-lamp electroliers exhibited were supplied by a firm bearing a name much honoured by electricians—Messrs. Faraday.

I have to thank you for the kind attention with which you have received my paper.

The  
President.

The PRESIDENT: I should mention that the discussion upon this paper will be adjourned at the close of this meeting, to be resumed at an Extraordinary Meeting proposed to be held on March 17th; so that I shall be glad to receive the names of those who would like to speak on that occasion. Dr. Gladstone's work has been referred to in the paper, and as that gentleman is present I ask him to favour us with any remarks in reply.

Dr.  
Gladstone.

Dr. J. H. GLADSTONE: I have listened with great attention and interest, as you may imagine, to this able and valuable paper. I think, however, that I shall best consult your feelings by not entering much into the general questions which are contained in it, but rather by speaking upon those points in which the opinion of the writer either agrees or disagrees with my own.

I should like, first of all, to thank Mr. Fitz-Gerald heartily for the kind way in which he has referred to the work of Mr. Tribe and myself, and for the readiness with which he has adopted our main conclusions, although at the same time he has criticised some of them.

I will take his criticisms in order. He first dwells upon the matter of local action—a matter, no doubt, of great interest to him, because in this lithanode battery the object has been to get rid of that as far as possible. At the very commencement he thinks

that we have made a *lapsus*, and he has been led to do so on account of the detestation in which he holds anything in the form of "white sulphate." Now, of course, one can sympathise with such a feeling; but at the same time I must assure Mr. Fitz-Gerald that had it not been for the formation of that "white sulphate" he never would have seen a Planté or a Faure battery at work, and therefore that, while it is no doubt sometimes an enemy in our batteries, it is a thing which is absolutely necessary, and which we must welcome also as a friend. Mr. Fitz-Gerald has quoted the inquiry and answer made in our book in regard to local action, and when I first read the quotation (for our Secretary was kind enough to send me a copy of the paper) it appeared singular that the author of the paper should not exactly comprehend our argument in regard to local action; but when he transferred the image in his mind to a picture upon the black-board, I at once saw the way in which he had been unable to understand us. There is not a flat plate of peroxide of lead resting upon the lead plate, but a powder, more or less crystalline,\* mixed up with dilute sulphuric acid, and touching the lead plate, not in one place only, but in myriads of places, as shown in the following sketch. Of course each piece conducts the current, as does also the surrounding sulphuric acid. Now suppose that, in the experiments which Mr. Tribe conducted in my laboratory, a zinc plate had been taken, with spongy copper on it, and placed in dilute sulphuric acid, we should have had such a commotion that it would have been like an explosion, and the whole matter would have been over in a few minutes. How is it that that does not take place when we have a plate of lead with peroxide of lead upon it which

Dr.  
Gladstone.



\* The powdered minium, as viewed under the microscope, consists of irregular amorphous fragments; when treated with sulphuric acid the peroxide produced is in very fine particles; if, however, it is produced electrolytically, little dark crystals of peroxide are found mixed with minute crystals of colourless transparent sulphate. Sometimes the sulphate forms a considerable mass of white salt studded over with the deep coloured crystals of the peroxide; and in some instances these crystals are large enough to be recognised as such by the naked eye.—J. H. G.

Dr.  
Gladstone.

is soaked with dilute sulphuric acid? We attribute that to the formation, at once, of a little film of sulphate of lead upon the metallic plate, which does not entirely stop, but impedes, the local action, as it is a very bad conductor; and that seems to be sufficient to account for the phenomenon. Mr. Fitz-Gerald says: "In my view, the electrolytic resistance in the local circuit would be diminished, instead of augmented, by interposing even an almost impermeable (but more or less moist) coating of sulphate of lead between the peroxide and its support, excepting at one or more points where the peroxide and the lead would be in perfect contact." Surely there would be no local action whatever if this sulphate of lead covered completely and absolutely the lead plate, so that the sulphuric acid could not get at it. It should be borne in mind that we did not suggest this explanation of the local action as a complete explanation of the phenomena, though, at any rate, I think it is one explanation of the reduced action.

It has been stated that a table is given in our book (page 6) which "appears to further disprove the figures given by the authors of the work referred to." It should be remembered that this is not an experiment with eight stages, but eight different experiments. Now such experiments are very difficult to perform: no two preparations, made with any amount of care, would be exactly alike; and this irregularity is evident in the results. I have noticed also in some other electrolytic decompositions that a reaction takes a little time in order to get into full swing.

A formula has been given in Mr. Fitz-Gerald's paper representing our views of the decomposition, and he approves generally of that formula. It has not been given quite in the form in which we printed it, but still it represents our view. We gave it as an electrolytic rather than a chemical formula: we represented the course of the reaction, while the chemical formula represents only the results of the reaction. I have nothing to find fault with in that, because it does express our view. What I do object to is Mr. Swinburne's stricture upon our

work. He wonders that chemists, when they take up these electrical questions, seem to lose their chemical knowledge; and he speaks as though we had supposed that a salt was a compound of an oxide with an acid. Well, I looked up our book, and find that that view is extremely carefully avoided. If we had been mere chemists, I do not think we should have fallen into that error, because it is antiquated chemistry; but as, I hope, we are more than mere chemists, I do not think there was any chance of our falling into it.

Many remarks of value have been made in regard to the incomplete reaction which takes place in the Faure and Planté cells, showing that we never get the full conversion into sulphate of lead. The fact is that the mixture of this sulphate of lead does interfere to such a large extent with the ready reduction of the oxide, and separates it so often from the lead support, that we never get the reaction complete, even with experiments extended over many days. With regard to the suggested formula containing  $Pb_2O_3$ , it is quite conceivable that some compound of that kind is formed; but, if I am not mistaken, Mr. Tribe looked very carefully indeed for any of these lower oxides—sub-oxide, sesqui-oxide, &c.—but was unable to find any indications of their existence. Where there is any divergence of opinion between the lecturer and ourselves, I can only say that I feel very much obliged for the way in which he has gone into this matter. We must not believe that any of our work is final. If we have arrived at a chemical formula of the reaction, we know that there are modifications which can easily happen. Then, also, there are great difficulties not yet solved—difficulties connected with the conducting power of these mixtures, and other things—which are very important when one comes to prepare these batteries for actual use. Doubtless the work of such gentlemen as Mr. Fitz-Gerald, who combine great practical knowledge of the subject along with a knowledge of chemistry, may enable us to get a true theory of these reversible lead batteries, and result in something which I hope we may all employ in our homes.

Mr. W. H. PREECE: It is quite evident that Mr. Fitz-Gerald, in this paper, has raised a question that will be discussed probably

Mr. Prescott. more from its chemical side than from its electrical or practical side; but it is because there is also a physical side, that deserves a little consideration, that I am tempted to occupy a few minutes in mentioning to you some results of my experience. I am a little surprised to find that not only Mr. Fitz-Gerald, but nearly every one who experiments with and uses secondary cells, neglects to avail himself of that useful little apparatus, the hydrometer. Now I have been using secondary cells practically for the past three years, and I have watched with the very greatest care their performance by means of hydrometers. The variation in the density of the solution of a cell is indicative of the chemical actions that go on there; and those who are interested in the question, and who are competent to make the calculations, can be supplied, if they like, with records that I have kept with a great deal of care for the past few years. My practice is this: In every cell there is placed a very carefully adjusted hydrometer, and readings of those hydrometers are taken every hour while the cells are being charged and every morning after the cells have been discharged. In the formula that Mr. Fitz-Gerald has given us— $\text{Pb O}_2 + 2 \text{H}_2 \text{SO}_4 + \text{Pb} = \text{Pb O} + \text{H}_2 \text{SO}_4 + \text{Pb SO}_4 + \text{H}_2 \text{O}$ —it is assumed that the cell has been charged; and when the cell is charged it is at its greatest density—it has dissolved in it the greatest quantity of sulphuric acid. When the cell is discharged it passes through a process by which this sulphuric acid, that gives it density, is transferred into sulphate of lead, abstracting from the solution that which gives it weight, and leaving behind virtually a smaller proportion of acid and water. Now the practice is to fill charged cells with a solution that has a density of 1,150, and after a little trial these cells acquire a density, when charged, which may be taken to be 1,210. Supposing our cells are charged, and have density of 1,210, then in my cells, which are 15 L. E.P.S. cells, there is a steady fall of one division, or one degree, for every 9 ampère-hours taken out; and it is possible and easy to indicate with absolute accuracy, to one ampère-hour, the quantity of charge that has been taken out of a cell and the quantity of charge that remains in. Usually the cells which were at 1,210 run down to about 1,190 before they are re-



charged; and it is found that in charging, for every 12 ampère-Mr. Preese. hours put in, an increment of one degree of density is observed on the index. The result is that this careful observation of density shows exactly what has been going on, and it shows unmistakably that the formula given by Mr. Fitz-Gerald is a true and accurate record of what takes place. But I want to point out that it is perfectly possible, by carefully calculating the variations in density and comparing them with the amount of sulphate that ought to be formed, that a test or check can be made on the accuracy of the chemical formula. Mr. Fitz-Gerald has an abhorrence of "white sulphate." I think that if he had as much experience as I have had with the actual practical use of storage cells of all kinds, he would find that this "white sulphate" was a necessity. It is quite impossible to find these cells without some indication of the presence of this sulphate. Sometimes, of course, it is in excess, and we then have that awkward operation going on called "sulphating;" and on this point I should like to mention that the plan of using carbonate of soda, mentioned by Mr. Barber Starkey in a communication to the *Electrical Review* a short time ago, has been tried by me with very great success. I went down to Mr. Barber Starkey's place near Bridgenorth to see what he had really done, and I found there that a set of cells that he had used badly had been very seriously sulphated, and by supplying a quantity of carbonate of soda he had succeeded in filling the cell with a solution of sulphate of soda, and by that means dissolved the abominable white sulphate of lead. I have repeated the experiment. I have had several cells charged with various proportions of sulphate of soda, and of sulphuric acid, and I find that there really is a certain proportion of soda which, if properly applied to a cell, will entirely remove all the outward and visible appearance of the white sulphate. But you cannot remove it entirely—it is essential to the action of the cell—and its presence is indicated, as I say, by accurate measurements of the density of the cell.

The discussion was then adjourned.



A ballot took place, at which the following candidates were elected :—

*Foreign Member :*

Joseph P. Davis.

*Member :*

George Frederick Pescod.

*Associates :*

George Darling.

Martin Hamilton Kilgour.

Charles Henry Raper.

Captain Anthony Thomson.

James Townsend.

S. N. Wilson.

Charles George Wright.

*Student :*

Henry William Handcock.

The meeting then adjourned until March 17th.

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An Extraordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday Evening, March 17th, 1887—Professor W. E. AYRTON, F.R.S., Vice-President, in the Chair.

The CHAIRMAN: In calling upon the Secretary to read the minutes of last meeting, I should explain that I occupy the chair to-night in consequence of a letter having been received from our President stating that he is confined to his room by illness, and consequently, much to his regret, unable to be present this evening.

The minutes of the last meeting were read and approved.

The CHAIRMAN: I have the painful duty of announcing the death of our Librarian, Mr. A. J. Frost, who was well known to all of us. The Secretary has kindly put into my hands some memoranda relating to Mr. Frost's services, but it is almost unnecessary to read them, because we all knew him so well in his connection with this Society—a connection which dates as far back as the time when he acted as assistant to the first Secretary. In April, 1877—ten years ago—he was engaged by the Council to prepare the Ronalds Catalogue for publication, and to arrange the Ronalds Library. Any of you—and I suppose that is all of you—who have seen the Ronalds Catalogue know what a great amount of labour must have been expended in preparing such a catalogue, and how valuable was that special knowledge which was possessed in such a high degree by Mr. Frost in the preparation of a catalogue so valuable as is the Ronalds Catalogue. I can speak from personal experience, having been for several years Chairman of the Library Committee, of the very great interest that Mr. Frost always showed in his work; in fact, as has been said more than once, he was a born librarian. When I became connected with the editing of the Society's Journal, he pointed out to me that there were many serious imperfections in the index to the old volumes, and informed me that at the Society of Librarians the index of our Journal was unfortunately taken as a sample to show how an index ought not to be made. He then made some suggestions as to how he thought an index should be made—suggestions that

I hope I profited by. The Secretary has reminded me that he published an index to the first ten volumes of the Society's Journal, which is, of course, of great value. I use it myself frequently: it saves one looking through the indexes during all the previous ten years in which the Journal has been published when one wishes to turn up any particular reference. Mr Frost was elected an associate in 1877. In 1880 the Ronalds Catalogue was completed, and the Ronalds Library Trustees were advised that the catalogue had been completed, and also the books bound as required by the terms of the trust. The trustees made an inspection, and certified that the Society had satisfactorily complied with the conditions of the trust. Mr. Frost was formally appointed Librarian to the Society in 1880. He completed the biographical memoir of Sir Francis Ronalds which appears in the catalogue. It is, in fact, perfectly clear that we have lost a very valuable officer of the Society in losing Mr. Frost; and I am sure you will all sympathise with myself and with the Council in the regret that we feel at this very serious loss that the Society has sustained.

The names of new candidates were announced and ordered to be suspended.

The adjourned discussion on Mr. Fitzgerald's paper on "Reversible Lead Batteries, and their Use for Electric Lighting," was resumed.

Professor G.  
Forbes.

Professor G. FORBES: The principal point to which I would direct the attention of the Society is in regard to the small table

#### UNION CELL.

RATE OF DISCHARGE. Ampères per Lb. of		CAPACITY. Ampère-Hours of	
Plate.	Cell.	Plate.	Cell.
·68	·41	9·2	5·5
·88	·53	7·1	4·3
1·2	·72	6·2	3·7
1·5	·9	5·5	3·3

which I have put up here, containing some facts relating to tests which I made upon this battery to which Mr. Fitz-Gerald has drawn attention. These tests show the rate of discharge and the capacity which corresponds with that rate of discharge. You will notice from the figures that when we diminish the rate of discharge we get an enormous increase of capacity. The curve which shows the capacity in terms of the rate of discharge is very remarkable indeed, rising very rapidly as the rate of discharge diminishes. My own tests which were made with this battery were purely with the object of finding its practical use, and I did not prosecute any scientific tests to find the ultimate value; but the form of that curve is so remarkable that I should like to ask Mr. Fitz-Gerald whether he has made any tests with extremely slow rates of discharge. The very lowest rates of discharge in my table are higher than that of any other secondary battery which has come under my notice for the same weight.

I made a good many tests of these batteries last year, and their lightness is of course a very remarkable feature about them: they are lighter for the capacity that they have than any other which I have ever seen. The material itself, which Mr. Fitz-Gerald has invented, is, as every member will see immediately, a very remarkable material indeed, and is mechanically good; it looks like the right sort of stuff, from its solidity, and it looks as if it were a durable material. Of course one cannot tell by simply making tests of a few months whether it is going to have a long life or not; but while I had these batteries under my own supervision I took care to look over all the loose plates that had, some of them, been in use for years, and they all, however old they were, seemed to me to be just as good as when they were new. Moreover, I never saw any white sulphate on them; so that I must say that I felt that Mr. Fitz-Gerald had made a very great stride indeed in secondary batteries in inventing this anode, and I am sure that the more tests are made of it the more they will be appreciated.

There are, however, some points in this battery independent of the lithanode. The manner in which it is supported in the cell

Professor G. Forbes. is different from all others, in the first place, in the employment of mercury for the contact, and, in the second place, in the employment of celluloid for the framework, and of platinum for making contact. As to the mercury, one has generally a sort of objection to using mercury for a permanent contact, but I do not think that it is a very serious objection; and really, as far as I see, it is only adopted because the inventors of this battery have thought that it was more convenient to have that mercury contact, in spite of such imperfections as it may sometimes possibly have, in preference to ordinary contacts, which are certainly liable to become imperfect sometimes and lead to a great deal of trouble; as all of us who have used secondary batteries know, we do have great trouble sometimes from imperfect contacts of the plates in a cell.

I should like if Mr. Fitz-Gerald could give us some little more information than he has about this celluloid, which, so far as I have been able to see, appears to be a good material for battery work, and seems to be very easily worked also. Of course the use of platinum for making contact is rather an objection on account of the expense, but that really seems to me to be the only defect in the whole battery.

I have said that the result of all my tests was most undoubtedly to lead me to think that Mr. Fitz-Gerald had made an enormous advance in secondary batteries. There is, however, one point that has puzzled me very much indeed—that is, as to its cause—but which seems to me to add a new feature to the form which is at present before us. You all know that when we are charging an ordinary secondary battery, an E.P.S. cell or any other, we have what can be called a spurious electro-motive force; *i.e.*, we have the natural electro-motive force of the battery, which is almost exactly 2 volts, but at the first movement after charging it we find that the electro-motive force of the cell is at least  $2\frac{1}{2}$  volts, sometimes a bit more than that; and it follows that during the whole of the latter time that we are charging up the cell we are charging against an electro-motive force of  $2\frac{1}{2}$  volts, and when we discharge it we discharge it at 2 volts, because this spurious electro-motive force disappears almost immediately.

Therefore, neglecting all other causes of loss of efficiency in a secondary battery, we have simply, owing to this cause in itself, a loss of efficiency in the ratio of  $2\frac{1}{2}$  to 2. Professor G. Forbes.

Now in the cell which is before us, in the whole of the tests which I have made, I have never seen any spurious electro-motive force such as I have mentioned. The highest electro-motive force which I have ever got from a cell immediately after charging it has been considerably under 2.1, and the terminal electro-motive force when I was charging it—that, of course, depending upon the resistance of the battery—was never in excess of 2.15. This, then, seems to me to be an enormous advantage that has been gained in this cell, viz., that instead of having a necessary loss from this cause, however much we may improve the battery, in the ratio of  $2 : 2\frac{1}{2}$ , you certainly have not more than 2.0 to about 2.1. This is a very remarkable thing. I have had a great many secondary batteries to test at different times, and I have never seen one doing the same as this. What is the reason of it I cannot say. The general assumption as to the cause of that spurious electro-motive force is that it is due to bubbles of hydrogen sticking to the plate. But if it is due to bubbles of hydrogen, then the depolarising action, as you may really call it, seems to be something like the action of the platinised plate of the Smee cell. The surface of the plate seems to have some action in allowing or not allowing of the attaching of hydrogen bubbles to the plate; probably smoothness of surface has something to do with it. I am sure that I, in common with everyone else, must congratulate Mr. Fitz-Gerald most heartily on having produced a plate that is certainly a great advance in secondary batteries.

Professor S. P. THOMPSON: I have, thanks to the kindness of the Primary Battery Company, had a small cell for some months under my charge in my laboratory, and from time to time I have charged it up and discharged it, and it has behaved extremely well, though it is not quite of so recent a type as the cells that are shown to the Society. It has behaved itself well, I am bound to say. It has not sulphated; it has not performed any awkward tricks; buckling has not occurred in the peroxide plates, but only slightly in the celluloid frames. There is no sign whatever Professor Thompson.

Professor  
Thompson.

of deterioration in the lithanode plates. I was, however, a little disappointed when I came to work with it, on finding that I could not get as large a discharge from it as I had expected in proportion to the surface of the plates. I do not want to speculate on what I have not really tested; but it seems to me that a higher rate of discharge for a given weight or surface of these cells would be obtained if the electrode, or, rather, the metal that is in connection with the lithanode, were of rather larger area. Now here one comes across a certain practical difficulty, because the inventor, with the express object of avoiding local action between the peroxide and the leaden frame, has adopted a platinum contact; and he has certainly very ingeniously reduced to a minimum cost that platinum support, for the two strips of platinum foil that go down behind the lithanode plate cannot cost very much. I do not say they will cost less than a leaden frame would cost, but they cannot cost very much. But I fear that in having such an extremely small amount of platinum in contact with the lithanode, something has been sacrificed. My impression is that lithanode is not so good a conductor that you can afford to introduce into the battery the resistance that it interposes in the path of the current in going from the edges of the plate to the platinum contacts. That resistance would be greatly reduced if you had a frame all round the edges of it, or a grid fixed through it, or if there were a more extensive surface of platinum in contact with the back. I believe that this interior resistance is the reason why one does not get such large currents as one would expect from that surface. If I am wrong, perhaps Mr. Fitz-Gerald will correct me; but I believe that with an equal amount of surface in the battery you cannot use the same discharge current as with the plates that have grids around them. If I am wrong I shall be glad to be corrected, but I did not find it so in the case of the little battery which he was kind enough to place at my disposal. I certainly saw no trace of the formation of white sulphate; but I must disagree with Mr. Fitz-Gerald entirely on the white sulphate question. I do not think there can be two opinions upon the essential badness of a plate that is all white with sulphate; *but we ought to bear in mind that there are two sulphates of*



lead, and that both these sulphates may occur in a secondary battery. There is the grey sulphate of lead, if I may call it so—a semi-transparent substance, grey, not white, when we find it newly formed in our batteries. It has the chemical formula  $PbSO_4$ , and is comparatively easy both to oxidise and to reduce; it is not a hopeless substance. But if this substance has been formed for some time in the cell—for example, by the ordinary process of charging and running down—and if you have carried the point of discharge so far that you also have Pb reduced to the state of monoxide, and if these two substances are simultaneously present in the cell, they very soon take the opportunity, when the cell is idle, of combining, and so you get a basic layer of lead ( $Pb, SO_2$ ). This is a whiter substance than the simple sulphate of lead, and if it grows to any extent in the cell is absolutely detrimental, for it is not reduced by the hydrogen at the reducing end, and it does not turn to peroxide at the other end. It simply chokes the cell and falls out in the form of a white powder. It is an utterly hopeless substance in cells.

There is a question which I would like to see discussed, upon which I think we really possess extremely little information. Suppose you have a fully charged cell: let us think about the peroxide end—I will not call it the positive, as I usually do, because Mr. Fitz-Gerald calls it the negative, and I am afraid we should misunderstand one another—I mean the plate that is of a ruddy-brown colour—the peroxide plate;—one cannot misunderstand that. If we have one of these fully charged ruddy-brown plates, and begin to discharge it, in what way does the discharge begin? We are having that go down to some lower state of oxidation, accompanied by partial sulphating: does it begin on the front surface, or does it begin next to the conducting support, whatever that may be, or does it begin simultaneously all through? I believe that if you have plates of one degree of density one thing may happen; if you have plates of another degree of density another thing may happen. Those pieces of lithanode are marvellously dense; I should expect that with the denser quality of lithanode the action would begin outside sooner than inside. I should expect that with a comparatively porous kind

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of density the action would begin close to the support, whether it was inside or whether it was at the back. The only piece of evidence, however, that I can offer on this point as to where the action begins, is from what may be seen in the first forming of a pasted cell. If one takes an ordinary piece of sheet lead and pastes it over with the preparatory paste of red lead and sulphuric acid, allows it to harden up to a certain extent, and then puts it into the cell, on charging it is either oxidised or reduced. In either case you find that there, unquestionably, the action begins next to the leaden core and proceeds from the middle outwards, and the outside is the last to be turned into peroxide or reduced to spongy lead. But I do not know—I have not really made experiments to tell—and I want to know, whether in the process of discharging, the reduction begins at the surface, or right through, or next to the support?

There is another matter that is, I think, worth discussing—that is, whether the scum, which was referred to last week by some speakers, that forms over the surface of the lead supports (and which some speakers believed, and I myself believe, to play an active part in protecting those supports from being bitten into and eventually destroyed) is simply sulphate of lead: we assumed last week that it was. But I have noticed again and again that the scum that you get is not of the true colour of the sulphate; it is generally of a dark brownish colour, but whether it is sulphate I do not know, and I want to have the opinions of others in that respect. There is an extremely beautiful experiment made by Dr. Oliver Lodge, recounted in his Cantor Lectures on secondary batteries, which bears a relation to this question of scum. He was recounting certain facts about Planté's old process of formation by charging, recharging, reversing and charging again, and he pointed out that if you take two plates of lead that have never been used before, and pass a current through them so as to slightly peroxidise one, the other, as we all know, and as Planté pointed out, does not get bitten into at first; hydrogen bubbles merely play over it, and its surface becomes slightly hydrogenised. Now if you discharge such a pair of plates, the reduced plate, having a mere film of hydrogen over it and no working depth of spongy lead, is

exhausted in a very few seconds. Dr. Oliver Lodge used two such strips to ring an electric bell, and he found that the action stopped almost suddenly after a few seconds, and the bell ceased ringing. Why? Not because the peroxide film had given up all its oxygen, but because the leaden plate had become covered over with the scum. He put a piece of zinc for a very short time—the tenth part of a second will answer—down into the liquid, and on touching the leaden plate the scum instantly disappeared, the plate resumed its metallic look, and the bell went on ringing for 10 or 15 seconds more, when the scum again formed. Apparently lead refuses to burn, or be consumed, in sulphuric acid when it presents a smooth surface; it requires to be made spongy, or at least to be finely divided at the surface.

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Thompson.

There are two or three remarks made by Mr. Preece last week that I wish to refer to incidentally. He urged the use of hydrometers in keeping a check upon the charge in the cell, and upon the general state of the cells. I can only too heartily endorse that recommendation. I know as a matter of fact that hydrometers have been used by the older experimenters—certainly by Mr. Sellon, and by the Electrical Power Storage Company for many years now. I think I am not mistaken when I say that the Electrical Power Storage Company has produced certain special little forms of hydrometer, made thin and narrow, so that they can be inserted between the plates of the cell. The hydrometer is a most excellent thing to work with; it affords a really scientific check upon the state of the cells. But when I endorse that remark of Mr. Preece's, I must entirely differ from the exact figures that he gave as those to which the hydrometer ought to read, and where he pointed out what variation should be allowed with a strength of acid of 1.210 (1,000 being taken as the density of water). Now the particular variation which he found in his cells is by no means the same that ought to be found in every cell. Observe, the variation of density in a cell depends entirely upon the withdrawal of acid from it. Now if your cell has a certain number of plates in it, and there is a certain bulk of acid, and you start with acid of a certain density, as soon as those plates are fully discharged they will have become to a certain

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extent sulphated, and a large quantity of acid will have been withdrawn. Well, I grant you that if you discharge these plates from some point to the same point, they will take up the same quantity of sulphuric acid whether they are in a big box or in a little box; but the change of density occasioned by the withdrawal of a given amount of acid from the water depends upon the actual amount of water in the cell; and Mr. Preece's figures, though right enough for one type of cell, must not be taken without some restriction as to what they mean.

Then, as regards the chemical formula which Mr. Fitz-Gerald wrote up for us, I accept it to a certain extent as representing chemical effects, but I think there is a good deal more to be said upon the chemical question. The chemical question is, however, capable of being settled upon an absolute basis. Why does an accumulator give an electro-motive force of about 2 volts? That is a purely thermo-chemical question. Why does the Daniell cell give us a little more than one volt? That is a purely thermo-chemical question, and can be shown without the slightest difficulty purely from thermo-chemical data, at any rate to a first approximation. For example, we know that, taking an equivalent quantity of zinc, and causing it to be consumed in sulphuric acid and form zinc sulphate, the quantity of heat given out from the zinc is 85,400 calories, whereas the similar data for the equivalent of copper show 37,520 calories, and the difference between the two is 47,880; and seeing that 46,000 calories or thereabout correspond to one volt, it follows that the E.M.F. of a zinc-copper element is a little over one volt. Now, if from the thermo-chemical properties of zinc and copper we can calculate the electro-motive force of a Daniell cell, we ought to be able to calculate precisely (if we know the chemical data) the electro-motive force of an accumulator. Conversely, if we know the electro-motive force of an accumulator to be 2 volts, or a little over or under as the case may be, we ought to be able by that very fact to say which of the various thermo-chemical data corresponds to that electro-motive force, and therefore say precisely what are the chemical facts.

What are the various hypotheses that have been propounded? Professor Thompson.  
I think I know of five.

Some people say that the electro-motive force of the accumulator is due to the reduction of peroxide of lead to spongy lead. It is rather a wild hypothesis, for it requires us to believe that the running down of spongy lead from the metallic state at the anode reduces the peroxide back to the metallic state at the other end!

The second hypothesis is that the peroxide is reduced from its condition of oxygen-activity by the sulphuric acid to become lead sulphate.

A third hypothesis is that at one end the lead goes to lead sulphate, and at the other end the peroxide is reduced to spongy lead.

The fourth hypothesis is that both the plates go to sulphate: at one end  $PbO_2$  gives up its oxygen and takes up  $SO_4$ , at the other end spongy lead takes up  $SO_4$ , and both become  $PbSO_4$ .

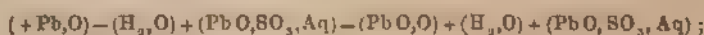
And the fifth hypothesis is that at one end the peroxide goes down virtually to monoxide, and that at the other end the spongy lead goes down to litharge also.

Of all these hypotheses which is correct? If we know the thermo-chemical data we can say absolutely which of these five is the proper hypothesis. It is only a question of knowing the facts. Take the first hypothesis—that peroxide of lead is reduced to spongy lead by hydrogen, by nascent hydrogen, from the water in the acid—what am I to write up? I have to write, not a chemical, but a thermo-chemical calculation. We have  $+(H_2O) - \frac{1}{2}(PbO, O) - \frac{1}{2}(Pb, O)$ . According to the researches of Tscheltzow the numbers are 68,800 — 6,070 — 25,530, and the net result is 37,200. Now 46,000 go to one volt or thereabouts, so these 37,200 only correspond to .81 volt; so that settles that that is not the action.

In the same way I could show you that the second hypothesis would lead to 1.17 volts. Also with the third hypothesis, which I believe is Mr. Fitz-Gerald's—that the spongy lead goes down to sulphate, and that the peroxide is reduced to a lower oxide: that would lead, according to the thermo-chemical data, to a force of less than one volt—.096.

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The accepted hypothesis is that both get sulphated. According to Tscheltzow the reaction is



now the numbers are

$$51,060 - 68,800 + 24,840 - 12,140 + 68,800 + 24,840 = 88,600,$$

which, turned into volts,  $\frac{88,600}{46,000} = 1.93$  volts, or very nearly

2 volts. This would show that the expression is a fair representation of what goes on. It is quite true that we get sulphate at both ends—I am perfectly sure on that point—and Mr. Fitz-Gerald will hear a further reason why he does not find it. But, although I say that, I am very strongly of the impression that sulphating at both ends is, in one sense, no necessary part of the action of the battery; it is necessary in another sense, because the elements are chemically what they are. Suppose you have zinc and put it into a battery, that zinc dissolves in the sulphuric acid and unquestionably gives out heat—that is, if you let it only do that; but if, instead, you let it drive a current through a wire, then the energy is carried off through the wire and gives out heat somewhere else. But does the greater part of the heat come from the zinc going down to the state of oxide of zinc, or does it come from the oxide of zinc going down to the state of sulphate? Remember also that if you have an identical action at both ends of the battery, that action will avail you nothing, even though it gives out heat. Suppose you have zinc at each end turned thus into zinc sulphate, there is heat at both ends, but they do nothing; the heat is non-adjutant, and does not assist the electro-motive force of the battery. Now, whatever else you have—the reduction of the oxide or the oxidation of the spongy lead—you certainly have lead sulphate; and if you have lead sulphate at each end the actions balance one another and become non-adjutant.

In the fifth hypothesis that sulphating is a necessary accident of a non-adjutant kind, and that the real actions are those that take place before sulphating begins—the heat values worked out give, as I make it, 103,460 calories, which is as near as possible 2.2 volts. Therefore I am inclined to think that that is the real action of the cells.



Now why does Mr. Fitz-Gerald distinctly object to our talking of the plates as sulphated? He says it is not sulphated. He has a larger quantity of lithanode—of  $PbO_2$ —than gets reduced down to a lower state; something like five times the quantity of  $PbO$ , that ever comes into play, or ever gives up its oxygen. It is the same in the Leclanché manganese cell as in the accumulator. You have a metal plate at one end and peroxide at the other. The peroxide of manganese gives up its oxygen to a small extent, leaving behind five or six times the actual weight of peroxide that actually comes into play. In the accumulator one-sixth only of the peroxide will change colour, and the rest will remain brown. All the higher oxides of lead are of low dark colours, and you may get an intermediate state of oxidation which is still of a dark colour. Further, the semi-transparency of the sulphate that is formed in working, will account for that not showing itself as a grey colour upon the surface of the plates.

One other explanation, please. I may be taken to task by some benighted chemist here for saying, or assuming, or believing it possible, that sulphating is a process which may take place in two stages; or that lead, zinc, copper, or anything else, can *first* oxidise and *then* sulphate. I shall be told, I suppose, as Dr. Gladstone seemed to hint last week, that that is an exploded chemical notion. A single experiment of Planté's will settle the doctrinaire chemist. Take two copper wires—two clean new copper wires—put them into dilute sulphuric acid; have two or three Grove cells or a couple of accumulators to give sufficient electro-motive force, and turn on a current, having a galvanometer in circuit. What happens? The instant the current is turned on a lot of hydrogen bubbles appear where the current goes out, and a small lot of oxygen at the other end; after about one and a half seconds the current, as observed in the galvanometer, goes down—it seems to stop—and if at that instant you look at the copper wire you will see that the oxygen has got covered with a brown surface. An instant afterwards the action begins again; the bubbles come off again, and if you look very carefully you will see dropping down from the surface of the oxidised plate a solution of blue sulphate of copper: there you have literally the



Professor  
Lampson.

oxidising action beginning before the sulphating begins. It is essentially an action taking place in two stages.

The  
Chairman.

The CHAIRMAN: I am reminded that we have to hear Mr. Fitz-Gerald's reply to-night; and as several gentlemen have given me their names as being desirous of speaking on the subject, it is necessary that they be as brief as possible.

Mr. Sellon.

Mr. J. S. SELLON: In answering the call of the Chairman, I feel under the disadvantage of not having been present when Mr. Fitz-Gerald's paper was read, and when some remarks were made by that distinguished worker in the field of secondary batteries, Dr. Gladstone. Indeed, it was not my intention or wish to take part in this discussion, lest it should be supposed that I was in any way hypercritical as to the work of others; and I think it right therefore to say that my one wish is to see improvements in the development of storage batteries, and I am only too glad that so many should be working in a direction which has so much to do with the progress of electric lighting.

Having said so much, I will rapidly glance over Mr. Fitz-Gerald's paper, which was courteously sent me. Passing over the preliminary remarks, I first come to the statement, with which I am obliged to disagree, as to the storage battery being still in its infancy. If so, it is certainly an infant of elephantine proportions. My view is rather that not only in its practical development, but I would, with regret, say, in its approach to perfection, it has passed the stage of infancy and arrived certainly nearer that of manhood. I use the expression "regret," because all of us would wish to feel that great improvements could still be made in the direction of greater capacity and lessened cost; but I fear that the only material points of improvement for which we can hope are those of better manufacture and better management in the use of the article we have at present to hand. I would strengthen that view by the fact that, although for years past the cream of electrical scientists throughout the world have been at work to this end, no great change has been made, and no great departure from the principles which were laid down in 1881 has resulted. As I have already said, it is only, in my opinion, to

greater care over their manufacture and in their manipulation Mr. Seaton. that we can hope for any much better effects.

With regard to the table of figures of the storage capacity of various secondary cells given by Mr. Fitz-Gerald, I can only say that the highest of them in no way surpasses those which were attained in 1881-1882. To this subject I will refer later on, when I will give some figures furnished by the Electrical Power Storage Company.

It is not my intention to trouble you with any formulæ on the subject of sulphating after the very able way in which Professor Thompson has dealt with it. The simple law which we have to deal with is the fact that as the peroxide of lead degenerates, by loss of oxygen, to a lower state of oxydation, and in exact proportion to those lower degrees, so must the sulphating take place; and neither argument nor variable method of manufacture can affect this law, which is entirely a chemical, not an electrical, one. Before passing on to the question of capacity, I will first allude to two points of practical interest, in fact the points for useful discussion raised in this paper—viz., the utilisation of solid peroxide plates as compared with packed plates; and, secondly, the practicability of furnishing the electric light to private houses by periodical supplies of storage batteries. Upon this last point I was forced to propound my views not long since, and they remain unchanged; in fact, the rough figures I then hastily put together give much latitude to the credit of the scheme. I did not nearly exhaust the list of difficulties which are to be encountered, but I will confine my objection to one simple point, making a present of the question of cost of supply, whether it proves to be one-sixteenth of a penny or one-sixteenth of a pound per lamp-hour.

It seems to be very imperfectly realised that the satisfactory working of a battery depends materially upon the uniformity of the electrolyte, that its strength should be properly adjusted, and that such adjustment should be maintained. Under the varying state of the replaced plates, as proposed by Mr. Fitz-Gerald, this becomes impracticable, and all sorts of disorders and difficulties will occur from this simple cause. Much as one would wish to

Mr. Bellon. see the daily delivery system flourish, we must, I fear, look upon it as out of the range of our present capabilities.

Now as to the utilisation of solid peroxide plates. It is stated that by their use larger capacity is obtained for a given weight, and that greater durability is also secured. My reply to that, and it is based upon results obtained from prolonged work many years ago, is that a great capacity under regular and normal conditions of work, though at first attainable, is not maintainable. The difficulties which supervene are those of the rapid clogging up of such plates, and the proportion of inert material, lower oxides and sulphate, which are essential to give stability to such a plate, is so great relatively to that of the useful active material, that there is no economy from the first starting point. The resistance throughout the mass is so varying, consequent upon the localisation of the conductor at one spot, as was pointed out by Dr. Thompson, that unequal work is brought about upon different parts of the plate. Those portions most distant from the contact connection being left in a different state of conversion to the nearer part, and thus the clogging up becomes aggravated, unless costly and difficult precautions are taken to avoid it. As a result the plates degenerate and deteriorate in a comparatively short time and lose their power of capacity. It would stand to reason that it is more advantageous to have a good conducting medium distributed evenly throughout the active material, such as is carried out with the grid form of plate, than to have only one strip of contact connection down the middle of the plate and a large mass of inert and comparatively non-conducting material throughout it. To sum up, the lead in the grid performs the doubly useful function of acting as a support and as a conductor to the active material, whereas the inert oxide and sulphate of lead in the solid form performs neither of these conditions satisfactorily.

As to the local action in the grid, it is a bugbear which I do not allow to be a factor in the argument. Its avoidance is simply a question of care in manufacture and in treatment, which the experience already gained ought to guard against.

The figures which I would wish to give you with reference to

the storage capacity of the E.P.S. secondary cells were only Mr. Sellon.  
furnished me just now. Having been absent on a journey, I was not able to refresh my memory from my old notes and I requested the engineer of the Electrical Power Storage Company to send me the laboratory records on the subject, and, with your permission, I will read his remarks, which I have just received. They are as follows: "It would appear that Mr. Fitz-Gerald has never taken an E.P.S. cell of the L type himself and given it a fair test against the lithanode, but he takes the full value of 41 lbs. of lead in the E.P.S., which would give the horse-power hour on Mr. Geere Howard's statement. I made an examination into the reports as to weight, &c., of this company's plates, and find that in a complete positive,—[Mr. Sellon: Of course he means the peroxide or positive pole plate]—weighing 5 lbs., the  $PbO_2$  in that form of paste weighs only 25 oz. Taking eight peroxide plates, we have 200 oz., or  $12\frac{1}{2}$  lbs. of peroxide to produce the horse-power hour—or, say, 375 ampère hours. This gives 30 ampère hours per lb. peroxide. Mr. Fitz-Gerald's lithanode, by his own statement, gives 16 ampère hours per lb.—the lithanode is therefore at a disadvantage with the peroxide contained in pasted plates in the proportion of about 2:1. Mr. Fitz-Gerald did not give the specific resistance of his material, but states that it contains about 70 per cent. of peroxide; and if the remainder consists simply of sulphate the specific resistance of the plate must be very high, and in addition to this there is the poor contact of the platinum strip after discharge for a short time. The normal output per lb., which is advised by us, may be exceeded, but our usual 'pass' tests of a set of batteries is when one L 15, taken at random, will easily give 400 ampère hours, when discharged, to 10 per cent. fall of E.M.F. This 400 ampère hours, divided by the weight of peroxide in the grid, gives about 30 ampère hours per lb., and it is evident, then, that we make twice as much use of our peroxide, even under the worst conditions, than Mr. Fitz-Gerald does of his lithanode under what we will presume to be the best conditions. When peroxide is tested, under the best possible conditions, in a grid, we find that 25 oz. would easily give 84 ampère hours." This is at the rate of

Mr. Sellon. 47 ampère hours per lb. of active material as against the 16 hours claimed for the lithanode. It therefore seems to me quite clear that one difficulty in working a solid peroxide plate is the difficulty of utilising any reasonable proportion of the material of which it is composed—a large portion of the interior, certainly four-fifths of the plate, soon becomes comparatively inactive, and the conductive power is of course diminished in proportion to its degeneration and sulphating.

Mr. F. V. ANDERSEN: I would like to make a few remarks upon the figures given in the paper. Mr. Sellon has already given some of the figures, which I wanted to mention, viz., those referring to the number of ampère hours per pound of peroxide. I had some time ago the opportunity of testing a large number of cells and found the figure for the E.P.S. 22-lbs. cell to be 2·26 lbs. of peroxide in the cell, with an output of 75 ampère hours, or at the rate of 33·2 ampère hours per pound of peroxide.

As to the rate of discharge Professor Forbes thinks that this battery would discharge rapidly. Now the paper shows the rate of discharge, which, as far as I can see, was meant to be the normal rate, as being ·257 ampères per pound of cell; and if we say that the average potential during the whole discharge is 1·95 volts, we see that the average rate of discharge is ·5 watts per pound of battery. Now, one horse-power is 746 watts, and there is half a watt rate per lb. of battery, so we find that the rate of discharge is  $\frac{1}{1492}$  horse-power for 1 lb. of battery, and con-

sequently we must use 1492 lbs. of battery when we want to work at a rate of one horse-power. I do not object to the main figures as regards weight per horse-power-hour, or storage capacity—the Union cell is supposed to have only 70 lbs. of battery per horse-power-hour, the 22-lb. E.P.S. cell 135 lbs. I must, however, remark that the latter figure, according to my experience, corresponds to a very high rate of discharge. The average weight by normal rate of discharge I found to be 110 lbs. But the E.P.S. 22-lbs cell, when working at its normal rate (certainly not too fast for its capacity), gives ·68 ampères, which means 1·33 watts per lb. of battery, so that there are 561 lbs. of battery to work at

the rate of one horse-power. Compare that with the 1492 lbs. of Mr. Sellon. battery for the Union. Of course, there is the greater storage capacity—natural, because it has no leaden grid in its negative plate; but the question is whether it is important to have a large storing capacity when it cannot work at a high rate. I should like to draw attention to the figures at the end of the paper, where it is proposed to use a battery which would work three 20-candle lamps for five hours. The stored energy required to do this is taken as = 1 horse-power-hour: then to burn for five hours requires the work to be done at the rate of one-fifth of a horse-power, and the battery would therefore weigh one-fifth of 1492, or 298 lbs. Now, 60 per cent. of the weight of this battery consists of the plates, so that the weight of plates required to do this work would be 179 lbs., or very nearly four times the 45 lbs. mentioned in the paper. If we now look at the E.P.S. 22-lbs. cell, we find that to work at the rate of one-fifth horse-power will require  $\frac{561}{5}$  lbs, or 112 lbs. of

battery, or, since only 50 per cent. of this battery is plates, 56 lbs. of plates. So we have to compare these two things: one battery weighs 56 lbs. in plates and the other weighs 179 lbs., the latter can store four times the energy required, but it requires to be so heavy to perform the work at the given rate. It may, of course, be a question, whether anything at all is really gained by avoiding having the lead-grid in these plates. As regards the capacity obtained by the peroxide being only one-half of what it is in other batteries, I think that the imperfect connection of the little platinum strip is sufficient to explain that. We could not expect that it would be able to work either for a long while or to receive a charge and give it out quickly.

Mr. BERNARD DRAKE: I think the chief question of interest to Mr. Drake, the general public with regard to secondary batteries, at the present moment, is first cost and durability. Time alone will show whether the plates exhibited here to-day will meet the requirements—namely, a cheap battery, which will give no trouble in working. Mr. Sellon complains that the progress made in secondary batteries has been slow; if this is so it is because



**Mr. Drake.** experiments made with new types of cell do not show results for many months, and care must be taken, even then, in working upon the results obtained from a single experiment or one is apt to fall into some error ten times worse than the one which is being rectified.

We have before us a table comparing the capacity of cells made by different makers, but it should be remembered that the output for any type of cells can be varied within wide limits. It was our usual custom, when with the E.P.S. Company, to modify the plates to meet special requirements, the output varying between four and six ampère hours per pound of plates. The figures given by Mr. Fitz-Gerald seem to include the connections as well as the plates themselves—these should not be considered, as they are often made larger than is absolutely necessary with a view to diminishing the total internal resistance of the battery. I am glad to see that Mr. Fitz-Gerald agrees with the theory propounded by Mr. Gorham and myself at the British Association as to the protection of the grid by a dense coating of peroxide and not by sulphate. It was due to this that we altered entirely the mode of treatment recommended to users of secondary batteries, and the results attained have shown that this modification has been successful. I am of opinion that the grid in a peroxide plate is almost, if not entirely, dry when in good order, and that local action sets in as soon as the protecting coat of peroxide is acted upon, which, I believe, is only the case when a cell is run out. Professor Forbes gives us a table in which he shows that the capacity of the lithanode plates varies considerably according to the rate of discharge. Has he taken into consideration the recuperative power of the cell? We made some cells for driving a torpedo, which were discharged at an abnormally high rate for a short time, and we found that the same curve of discharge could be obtained three or four times over if the cell was allowed sufficient time to recover itself. I am inclined to think that, provided successive discharges are taken, it will be found that the total capacity is not materially affected by the rate of discharge. The next question is the carrying about of accumulators for lighting purposes, and charging them at a central



station. This is much the same as the temporary lighting, of *Mr. Drake.* which we carried out a very large amount, having some twenty-five tons of cells constantly employed during the London season for this purpose. One is apt not to make sufficient allowance for contingencies in this work, and, although the actual cost of one single installation seemed to show that the system could be worked at such a price as would admit of its general employment, yet, when the total cost was taken for the whole season, we found that a remunerative price to us would be quite prohibitive to the consumer. Professor Forbes states that during the charge the E.M.F. of lithanode plates does not rise in the same way as with the grid type of cells. This is a most interesting point, and the reason is certainly not apparent. I should much like to know whether these cells have the same regulating property as the grid type, for this is not merely a question of internal resistance. Exhausted batteries will not regulate to anything like the same extent as when charged above the point at which the electromotive force commences its sudden rise.

Professor Thompson has given us an interesting explanation of the action in a battery. I think, however, he misunderstood the tenour of Mr. Preece's remarks. Mr. Preece did not state that the use of hydrometers in accumulators was anything new, but simply that if the readings were watched more carefully it would form a useful indication as to the chemical action which was taking place; also I would remark that the range of specific gravity is, as Mr. Preece said, the same in all the sizes of E.P.S. cells, regardless of the number of plates, for the proportion of the volumes of plates to acid was kept the same throughout all the sizes.

Professor S. P. THOMPSON: The cells would not necessarily be *Professor Thompson.* by the same maker.

Mr. BERNARD DRAKE: I understood you to say the E.P.S. *Mr. Drake.* cells.

Mr. W. H. TASKER: Professor Thompson has drawn attention *Mr. Tasker.* to, what he considers, the low rate of discharge of the cell before the Society. I think that when you come to compare the rate of discharge for the surface of the plates with that of other cells it

Mr. Tasker. will be found that, as it is equivalent to '016 ampère per square inch, it is at least as high as any other battery.

Professor  
Ayrton.

Professor W. E. AYRTON: Before calling upon the author of the paper to reply I would venture to make a few remarks, because my colleague and myself have been connected with the testing of accumulators from their earliest introduction in England—in fact the nameless results given in Mr. Fitzgerald's Table I. for the Faure cells are the results of some experiments that we ourselves made in 1881.

Mr. Drake has said, what, in fact, I sent for this book to enable me to say, with reference to the question as to whether the storage capacity per cell was really greater or not when the rate of discharge was varied. It is, of course, impossible to criticise these results in the absence of Professor Forbes, but I think that Mr. Drake's suggestion is probably the correct one, and that it is possible that Professor Forbes neglected the resuscitating power. For example, in a paper read by Professor Perry and myself before the Physical Society in January, 1882, relating to some experiments on the Faure accumulator, we said, "An insulation of a few hours will cause the energy given off per minute on recharging to be eight to ten times as great as it was before insulation. Indeed on one occasion, after a cell had apparently discharged itself, it was left short-circuited with a thick wire for half-an-hour, then insulated all night, when the number of foot-pounds of work per minute given off at the commencement of the discharge the following morning was found to be ten times as great as it was on the previous evening, and a greater amount of energy was actually taken from it in the second discharge than in the first."\* Of course these tests referred to rapid discharges.

If you want to find out, therefore, the storage capacity of a cell it is not merely sufficient to wait until it gives off no electricity, you must insulate it for some hours, and, as Mr. Drake has said, start it again and see whether the next, or the fourth or fifth, discharge is at all comparable with the first.

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\* *Proceedings of the Physical Society of London*, Vol. V. page 107.

I should like to ask whether in these cells it is found, as one finds with the E.P.S. cells, that the discharge remains nearly steady for a very long time, and then (as I have found in several cases) it drops almost suddenly, as if there was an emptying of the cell? That is sometimes delusive, as there is still more to come out, but I should like to know if there is a gradual or sudden falling off in the discharge, and whether, when it does fall off, it is due mainly to an increase of resistance or to a diminution of electro-motive force.

Professor  
Ayrton.

We are most deeply indebted to the Electrical Power Storage Company for the energetic way in which they took up a rather hopeless problem and made a very great success of it. I remember how Professor Perry and I were pooh-poohed, in 1881, for believing in the importance and the future of accumulators. A well-known electric light engineer told us "a steam engine and boiler is the best accumulator." Well, that engineer sells accumulators now. But while expressing my deep gratitude to the Electrical Power Storage Company I have to criticise something—it is a detail—and that is their introduction of the expression "horse-power-hour capacity." That is, I venture to think, a most delusive expression, because it really does not tell you anything. It is very much as if one said when buying a watch that one was living at the rate of £100,000 a year for a minute. That gave no idea of what one's income was. Why horse-power-hour, why not horse-power-minute or horse-power-second? We do not want to know what is the horse-power that a cell could theoretically give off if it could be discharged in an hour, because it is physically impossible to take all the energy out of a cell in that time. What we want to know is, at what rate you can discharge the cell safely, what rate the maker will allow the cell to be discharged, how many amperes per lb. weight can be taken from the cell, or, better still, how many watts per lb. (because that takes into account the electro-motive force), and for how long a time can this number of watts per lb. be taken. I am not sure whether per pound sterling instead of per lb. weight would not be better, because that is cost we are really concerned with. We want to know for every sovereign of prime

Professor  
Ayrton.

cost what rate of discharge in watts is obtainable, and for how long a time. Such information would be infinitely more valuable than saying that a certain accumulator (which really required, perhaps, eight hours to be discharged economically) had a storage capacity of a million horse-power-seconds.

I would refer to one remark made by Dr. Thompson, when he spoke about what is the exact chemical action which takes place in the cell. I feel sure that my disagreeing with him is probably because I misunderstood him, and therefore, perhaps, he can put an additional word in when looking over the transcript. Does he think that at the beginning, when a cell is discharged, there is a sulphate formed on both plates, or only after a certain time? If there be not a sulphate formed on both plates at the beginning, then, of course, it is quite right to take a different chemical action for the discharge at the beginning from that taken after a certain time. But if he considers that there is a sulphate formed on both plates, even at the beginning of a discharge, then I do not see that it helps him much to consider the two stages in which the sulphating may be supposed to form, because you have not merely to consider the first change, the  $\text{Pb O}_2$  changing into  $\text{Pb O}$ , and  $\text{Pb}$  into  $\text{Pb O}$ , but you have to consider the second action, viz., the sulphating of what remains, and if that is taken into the account it is the same as his fifth hypothesis where the sulphate is formed at first. Of course what I have just said disappears at once if he takes the view (and he probably does from what he has said) that the sulphate is not formed at all at first. I have no doubt he will make this point quite clear in his remarks.

Of course Mr. Drake is quite right in saying that it is very difficult to make money by carting about accumulators. I think that in one point he may be possibly misunderstood, and therefore perhaps I may be allowed to say a word in regard to his remark that it does not pay on a large scale. Usually things do pay on a large, or may very often pay on a large, scale which do not pay on a small scale. But I think Mr. Drake's meaning was probably this: In certain cases people who want the electric light in a house for *a ball, or other special purpose*, are willing to pay a relatively

large sum for one evening, and under such circumstances it would pay quite well to cart accumulators about, but there are not a sufficient number of people willing to pay large sums for temporary electric lighting to make it pay on a large scale. Mr. Fitz-Gerald refers to the ha'p'orth of milk delivery at the house-door in evidence of the possible commercial success of accumulator cartage, but he forgets that when the milkman comes round (at least to my house), he does not take the milk back again, he takes the can, and I will allow that the can has some weight, but the milk, which has the greatest weight, is left behind. Now, when you take back an accumulator you are taking back as much weight as you brought, and the weight of the useless thing is infinitely great compared with the weight of the useful thing, which is the power, and which cannot be weighed at all of course. The only case that I can think of at this moment—of carting about a useful thing in a vessel of which the weight is enormously great compared with the weight of the thing required—is that of sending about compressed gas. The gas is sent in very heavy iron bottles, and the difference of weight between an empty bottle and a full bottle is almost inconsiderable compared with the weight of the bottle itself. But then the sale of compressed gas is a very small industry compared with the milk industry, and it is exactly analogous with the industry of carting accumulators for lighting ball rooms. It is almost like buying a precious jewel, the weight of which is very small compared with the case containing it.

Professor  
Ayrton.

As to the names, I would suggest, for the benefit of the Nomenclature Committee, that it might be well if they could settle what should be the names given to the poles or plates used in an accumulator. The difficulty arises from the fact that the current goes both ways in an accumulator, one way in charging and one way in discharging; some people think of the charging and others of the discharging, and therefore the names are alternated. I am not sure that even Faraday, great as he was and clever as he was even in suggesting names, was particularly happy in the names "anode" and "cathode," even when applied to voltameters. I suppose that these names were suggested to him by his using

Professor  
Ayrton.

the old form of voltameter, a most inconvenient form, in which the wires came up through the bottom, so that the current on entering "came up" and on leaving "went down." But in the modern form of voltameter the wires frequently enter by the top of the instrument, so that the current goes down by the anode, or "up road," and up by the cathode or "down road." And if there be this difficulty with ordinary voltameters, how much greater must the difficulty be in naming the terminals of accumulators in connection with which we have to speak, not merely of the direction of the charging current but also of the discharging. Probably the solution will be found in dropping both the names anode and cathode, and probably also the names positive and negative, and in suggesting two other names altogether, one of which refers to the one plate and the other to the other plate; the plates do not change, and therefore if we have a definite name for each plate there need not be any confusion whether we be talking of charging or discharging.

Mr. Fitz-  
Gerald.

Mr. D. FITZ-GERALD, in reply, said: The most important thing that I have to say this evening is to correct an error that I am told I unfortunately made at the end of my paper. I referred to the weight of the battery boxes (which are still under the table) as 10 lbs., or a little over 10 lbs. I meant to say, double that weight, 20 lbs., or a little over.

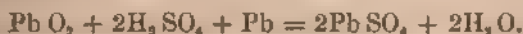
I suppose nobody here expects that I should answer all the points of interest, or even a large percentage of the points of interest, which have been brought forward in this discussion. If anybody does, he will certainly be disappointed. I feel myself as though I should require about three days to take in everything that has been said, instead of being in a position to refer to the points of interest within 10 minutes of time.

I made a few notes, however, in relation to the remarks made at the last meeting by Dr. Gladstone and by Mr. Preece. I come to the conclusion that Dr. Gladstone and myself are substantially in agreement. I seem to have misunderstood the meaning of the words used by him, "forms an almost impermeable coating between it (the peroxide) and the first metallic plate," and in my *paper I admitted* that the clogging up of the interstices in the



peroxide might be beneficial in reducing the local action. As Mr. Fife-Gerald, Dr. Gladstone has said, there may be more than one explanation of the phenomena observed, and I think we may safely conclude that, in the case of peroxide elements with a lead support, local action is impeded, when the peroxide is not impervious to the electrolyte, by the formation of lead sulphide in the openings or interstices through which the electrolyte obtains access to the lead plate. Or, again, local action may be impeded (as in the case of the wonderful Planté cell mentioned by Mr. Barker, and as in the plan advocated by Messrs. Drake and Gorham) by the formation of a very impervious layer of the peroxide.

With regard to another matter, I think we may safely accept the formula of Dr. Gladstone and Mr. Tribe, with one modification, as the electro-chemical formula representing the *initial* action occurring in the discharge of a lead secondary cell—the action which subsequently becomes complicated by another or others, as yet only guessed at, which have the effect of reducing the E.M.F. of cell. The modification in the formula that I refer to is that of taking hydrated sulphuric acid, instead of water, as the electrolyte, and admitting consequently that the spongy lead of the positive element enters into combination directly, instead of indirectly, with the acid radical  $\text{SO}_4$ . The result is still expressed by the equation



In regard to the remarks made by Mr. Preece. I fully appreciate the importance, theoretical as well as practical, of careful observations with the hydrometer and the voltameter simultaneously; although the former instrument has not been often used by me in my methods of investigation. Such observations indubitably might throw light on some of the questions which I have suggested, especially on the question as to the quantity of acid which enters into combination with the lead at the peroxide plate. Mr. Preece intimated that a more extended experience of storage cells on my part would prove to me that the formation of white sulphate—on the negative element I presume—is a necessity. But, as I have endeavoured to explain, my experience, at least with my own negative plates,

Mr. Fitz-  
Gerald.

is in a contrary direction. I have found that the production of "white sulphate" (non-conducting or unmixed sulphate) is *not* a necessity. I have charged and recharged sixty or eighty times the same lithanode elements, and, as others have also noticed, there was not any formation of this non-conducting sulphate.

As Professor Silvanus Thompson has pointed out, sulphate is undoubtedly formed in the plates, and it can be dissolved out; but it is produced in admixture with peroxide of lead, so that the plate never loses its conductivity, and the sulphate is in a condition to be readily peroxydised instead of being in compact masses, which, as we all know, are extremely difficult to peroxydise or to reduce.

I do not think I have any observations to make in regard to Professor Forbes's remarks.

But I think before we leave I ought to mention a point as to the rate of discharge of the lithanode elements—namely, that it is susceptible of being varied within wide limits.

I was asked whether I had ever discharged the plates at an extremely low rate: I really cannot say that I have done so. I have almost invariably had in my mind the necessity for discharging them with some degree of rapidity, but still with a degree of rapidity which did not involve a large expenditure in platinum. According to the conductivity of the plates, and to the surface of platinum employed in establishing the contact, so will the rate of discharge that is economically practicable become augmented.

I do not know whether I have correctly apprehended some of the statements which have been made in reference to the E.P.S. cells; but I should be surprised to understand—to have it brought home to me—that either the storage capacity or the rate of discharge of any other battery is higher than, or as high as, those which have been stated by Professor Forbes to belong to the lithanode battery.

I must now conclude my observations—perhaps in the Journal of the Society I may be allowed to advert to some of the other observations which have been made.

A hearty vote of thanks was accorded Mr. Fitz-Gerald for his paper.

*The meeting then adjourned until March 24th.*

The One Hundred and Sixty-fifth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 24th, 1887. Sir CHARLES T. BRIGHT, M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council, viz. :—

From the class of Associates to that of Members—

William Lynd.

Charles Bright.

W. H. Snell.

E. Stallibrass.

A donation to the Library of the Society was announced as having been received since the last meeting from the American Bell Telephone Company, to whom the thanks of the meeting were unanimously voted.

The Secretary then read the following paper:—

## THE RESISTANCE OF FAULTS IN SUBMARINE CABLES.

By A. E. KENNELLY, Associate.

Foremost among the problems which occupy the attention and tax the ingenuity of Telegraph-Engineers and Electricians is that which relates to the measurement of the resistance in, and up to, such faults as may develop in the circuits conveying their currents; and the accuracy with which this measurement can be conducted is a subject of great technical, and frequently of considerable economic, interest. New facts, therefore, which tend to elucidate the phenomena that faults present, or new

methods that lend aid to the measurement of their quantities, need no apology for their presentation to those practically interested in telegraph work. It is with the view and in the hope of aiding in both those directions that the present paper is submitted to this Society.

Although there can be little doubt that the laws which are about to be stated control all faults in sea water, yet their practical application will be found to be limited to large, open exposures, such as occur in the majority of cases where cables are totally broken. On this account we shall confine our attention to "total breaks."

Although there are many different methods in use for the measurement and elimination of the resistances of faults, or partial earths, in cables, there has been up to the present time apparently only one published method for measuring the resistance which the broken end of a cable offers, and that—due, we think, to Mr. H. R. Kempe—is based upon the measurement of the amount of discharge from the cable immediately after a balance has been obtained by Wheatstone's bridge.

The difficulty which is so frequently met in executing this test is the elimination of the rapidly falling polarisation current from the first throw of the galvanometer needle, which should register discharge only. The formulæ for separating the effects upon a galvanometer needle, of a steady current from a discharge current combined, are published in the Journals of the Society; but when the polarisation current is a large fraction of the discharge, its rapid fall in intensity still further complicates the subject.

The course, therefore, almost invariably adopted for localising a break by the tests from one end of the cable only is, as we all know, to reduce the break's resistance by suitable methods, such as Lumsden's, to an apparent practicable minimum, and then to estimate the value of this minimum from the general "behaviour" of the break, *i.e.*, its variability under varying testing currents.

In practice, the most common minimum resistance of a break arrived at in this way may be placed between  $10\omega$  and  $40\omega$ ; less

frequently between  $5\omega$  and  $75\omega$ ; while in rare cases the limits zero and infinity are approached. In the former case the conductor is in firm metallic communication with the sheathing of the cable, and in the latter the end of the conductor is so nearly enveloped by its gutta-percha covering that the hydrogen evolved in testing completes the seal.

Although an experienced judgment very frequently enables an observer to estimate accurately the resistance of a break, still not only is this a very unscientific means of arriving at the result in the present condition of electrical knowledge, but occasionally accidental circumstances may interfere with the accuracy of these estimates, and it would be a great advantage gained for an observer if he were enabled to bring actual measurement as a check upon his judgment.

Besides the above-mentioned method by discharge measurement, various expedients appear to have been brought forward at different times with the object of comparing the relative variations in resistance that different breaks exhibit with different battery powers, and so arriving at comparative results for any given break. Mr. Latimer Clark in the first instance, in 1862, and Sir Henry Mance so lately as May, 1884, have published tables of the relative resistances of different exposures, under certain conditions of electro-motive force, resistance, and time. In fact, formulæ have been prepared based upon such experiments—notably, we believe, by Mr. R. K. Winter and Mr. B. Smith. It is probable that the necessarily empiric character of such formulæ, not claiming to be based on any generally recognised law of electricity, has stood in the way of their wider acceptance.

Underlying all these facts there seems to have been the consciousness of an unexpressed law. It has long been known that the resistance of a fault diminishes when the current-strength passing through it increases, so long as the exposed surface is maintained constant; but the rate of the diminution for a given current increase seems to have hitherto evaded inquiry. Still it is only consistent with all the accepted doctrines of energy that the rate of diminution must be in obedience to some particular

mathematical laws. In fact, Mr. Latimer Clark's words may be quoted from his remarks on Sir Henry Mance's paper of May, 1884, already referred to: "There evidently is a law, or set of laws, running through the figures, which could be put into the shape of formulæ, and made of use to the practical electrician."

The results of a series of experiments extending over several months, made with a view to studying the rate of diminution in resistance for increasing current-strengths, seem to show that all faults obey two simple, mutually supporting, conjugate laws, whose effects, however, in practice appear to be greatly masked and encroached upon by disturbing influences of a physical nature. It is therefore desirable to examine the subject from its theoretical aspect at first; and when the truth of these laws is represented, we can proceed in confidence to seek how far their operation can be practically depended upon.

The two laws may be stated thus (they refer to one direction of current at a time only):—

- I. When the exposed area is constant, the current-strengths passing through the exposure are inversely proportional to the squares of the resistances which the exposure respectively offers.
- II. When the current-strength passing through the exposure is constant, the areas of exposure are inversely proportional to the squares of the resistances which the exposure respectively offers.

These are laws of inverse squares. For practical purposes the following enunciation in terms of inverse square roots is preferable:—

- I. The resistances of a constant area vary inversely as the square roots of the current-strengths.
- II. The resistances to a constant current vary inversely as the square roots of the exposed areas.

So that, if these laws are true, we halve the resistance of an



exposure either by quadrupling the strength of the traversing current, or, if the current remains unaltered, by quadrupling the exposed area.

It is easily seen that the demonstration of one of these laws causes the truth of the other to follow as a corollary. For assuming, say, the first law, and that a certain exposed surface ( $s$ ) offers a resistance  $r$  to a particular current-strength ( $c$ ), then when another current,  $\frac{c}{n}$  (where  $n$  is not necessarily an integer), passes through the exposure, its resistance becomes, by hypothesis,

$$r\sqrt{n}.$$

Now suppose that the original current  $c$  is maintained unaltered, but that the surface of exposure  $s$  is altered to  $ns$  (where the value of  $n$  is independent of that assigned in the previous expression), then the strength of current passing through the exposure per area  $s$  has changed from  $c$  to  $\frac{c}{n}$ , and we have seen that the resistance of each area  $s$  must have become  $r\sqrt{n}$  in consequence. But there are  $n$  of these areas, connected together in multiple arc, and their combined resistance—that is, the resistance of the whole surface

$$\begin{aligned} ns &= \frac{r\sqrt{n}}{n} \\ &= \frac{r}{\sqrt{n}}. \end{aligned}$$

The form of this equation proves that the resistance varies inversely as the square root of the exposed area—the second law.

It is interesting to observe that among all the functions of  $n$  by which we could conceive a law to take effect, the square root of  $n$  is the only one that makes the two laws conjugate as above. For suppose it were found to be true by experiment that the resistance of an exposed surface did not vary as the inverse square roots, but as the inverse cube roots, of the current strengths traversing it, then it would follow that with a constant current the resistance of an exposure would vary inversely, not as the square root or cube root, but as the cube root squared, of the exposed area.

For, by the above reasoning, if when  $s$  is constant, the resistance with  $n c = \frac{r}{n}$ , then when  $c$  is constant the resistance of  $n s = \frac{r n^2}{n} = \frac{r}{n^2}$ ; and so on.

It becomes, therefore, only necessary to confine our experiments to the measurement of the resistances offered by varying exposures to the same current in order to arrive at the law of the resistances offered by a constant exposure to varying currents, and perhaps the former is the simpler experimental problem. But, whichever plan is followed, the great source of error and difficulty in making these experiments is found, as might be expected, in polarisation.

It interferes, as we all know, in three ways—1st, mechanically, because when either gas or a metallic salt accumulates on the exposure, the area exposed to free liquid contact is reduced to an extent that it is impossible or very difficult to ascertain; 2nd, electrically, because the gas generated opposes a varying electro-motive force to the testing current, which is thus altered from the strength calculated for, and the exact correction is difficult to arrive at; 3rd, electrically, because this electro-motive force of polarisation existing in the circuit whose resistance is to be measured causes a large error to be made in the measurement, and owing to the great rapidity with which it falls, the moment the testing current ceases, the exact correction to apply is difficult to obtain.

Of these three errors the last is generally by far the greatest, and has probably been chiefly instrumental in enabling the laws of fault resistance to evade exact inquiry.

If the polarisation electro-motive force were constant, or if it varied at any known rate, it would of course be readily possible to find the extent of the third error in any given balance or measurement. But the rate at which it falls after the cessation of the testing current is apparently not clearly known, and would seem to be dependent on chemical conditions, the elements of the charging and discharging circuits, and time. Professors *Ayrton and Perry*, in a very interesting communication to this

Society's Journal (Volume V.), have recorded observations on the subject, but they mention that during the first few seconds after the removal of the testing current, in their experiments, the fall of polarisation electro-motive force was exceedingly great, and beyond the registering capabilities of the apparatus they employed. In fact, apart from all electrical fall of potential by discharge, if this polarisation electro-motive force has its chief seat in the contact of the developing gas with the metallic surface of exposure, which gas is continually being given off either in bubbles or in solution, then on the cessation of the testing current, as soon as the gas last developed has had time to quit the surface of the exposure—no doubt for a large quantity of it only a very brief interval—the electro-motive force due to the presence of that gas has disappeared from the circuit.

The following experiment will show how greatly the interval of time between the cessation of the testing current and the observation of the polarisation electro-motive force for correction, affects the results obtained.

It may be premised that all the measurements in this paper have been taken with the Wheatstone's bridge, and the corrections for the third polarisation error mentioned above have been all effected by reading to the polarisation current as zero, or by what is commonly called "false-zero" reading.

#### *Experiment.*

From a sheet of tinned iron 0.5 mm. thick, eight plates were cut, each approximately 15 cms.  $\times$  2 cms., with a copper wire soldered to one end. They were then immersed in salt water, by means of a suitable frame, to about 7 cms. in depth, each plate thus exposing about 28 sq. cms. of surface to the water.

The resistances of various combinations of these exposures were then measured directly by Wheatstone's bridge, with a current maintained at 10 milliampères in the exposure arm. By means of a metronome striking a bell at one, two, or more seconds, the time intervals were appreciated between the cessation of the testing current and the observation of the polarisation "false zero."

The table shows the comparative resistances measured in this way, the zinc pole of the battery being to bridge.

Table I.

No. of Series.	Strength of Current through Total Exposure.	Interval of Time between Cessation of Testing Current and the Observation of the False Zero of Polarisation.	Number of Plates connected in Parallel Circuit, and the corresponding Resistance observed.								Mean Coefficient of Resistance in each Series for a Doubled Area of Exposure.
			1	2	3	4	5	6	7	8	
	Milli. amperes.	Seconds.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	
1	10	...	12.5	9.1	7.5	6.3	5.3	5.3	4.9	4.6	0.714
2	"	1	48	43	39.5	36	33	29	26	22	0.770
3	"	2	55	52	48	44.5	39	34.5	31.5	28	0.789
4	"	3	62	57.2	52.5	48.5	43	41	39	36.5	0.826
5	"	4	64.5	59.5	53	49	46.5	44.5	42	39	0.845
6	"	5	66	61.5	56	52	48	45.5	43	40.5	0.842

Here the apparent resistance of exposures with 1, 2, 3, . . . to 8 plates—corresponding to about 28, 56, 84, . . . to 224 square centimètres,—is shown in respect to time. Series No. 1 is supposed to have been observed at an exceedingly small interval after the cessation of the battery current, in a manner to be presently described. It may be called the series at 0 seconds, or the series taken to "immediate false zero." The false zero so observed would therefore be the correct measure of the polarisation electro-motive force during the last flow of the testing current, and consequently the observed resistances in this series No. 1 should be the actual resistances which that current encountered. It will be seen that the results obtained in this series follow the law of inverse square roots closely; that is to say, the resistance is approximately reduced one-half by quadrupling the exposed surface. The coefficient of resistance reduction for a doubled surface by inverse square roots is of course  $\frac{1}{\sqrt{2}} = 0.707$ , and the mean coefficient in this observed series is

$$\frac{9.1}{12.5} + \frac{6.3}{9.1} + \frac{5.3}{7.5} + \frac{4.6}{6.3} = 0.714.$$

4

The difference between the observed and calculated coefficients is 0.007, or about 1 per cent. This discrepancy may be accounted for either by error of observation, by the interval of time between the cessation of the testing current and the false-zero observation not approaching zero closely enough, or by any extraneous resistance in the exposure circuit between the face of the liquid envelope and earth. The last two sources of error would inevitably increase the observed coefficient.

Series No. 2 was taken with a false zero, observed at one second's interval after the testing current ceased. The table shows that the balanced resistances have, by this means of observing, risen 300 or 400 per cent. The mean coefficient of resistance reduction for a doubled surface is now 0.770, or about 10 per cent. more than the law of inverse square roots would warrant.

Series No. 3 was observed to a false zero, two seconds after the cessation of the battery current. The observed resistances are now 400 or 500 per cent. above the corresponding resistances in series No. 1, and the mean coefficient for doubled surfaces is now apparently 0.789, which is nearly what would be expected if the resistance varied, not as inverse square roots, but as the inverse cube roots of the exposed surface, namely,  $\sqrt[3]{2}$ , or 0.794.

In series No. 4, taken to a false zero at three seconds, the apparent resistances are 400 to 700 per cent. above the corresponding resistances in series No. 1, and the mean coefficient for doubled surfaces appears to be 0.826.

In series No. 5, taken to false zero at four seconds, the difference in the readings from the corresponding observations in the preceding series is less than has previously been the case; that is to say, the fall of polarisation electro-motive force rapidly slackens after three seconds have elapsed from the cessation of the testing current. The mean coefficient for doubled surfaces now appears to be 0.845, or about what would be expected from a law of inverse fourth roots, namely,  $\sqrt[4]{2}$  or 0.841.

The same remarks apply to series No. 6.

In the tables appended to Sir Henry Mance's paper above mentioned, appearing in No. 53 of the Society's Journal, the observed coefficient for doubled surfaces of exposure is seen to vary between 0·80 and 0·95. The current-strengths, however, do not appear to have been constant during the different series, so that it is difficult to establish any exact line of comparison between those and the results of Table L.

In observing such a series as No. 2, when for a particular exposure a balance has been obtained, we notice, on releasing the key that applies the testing current, that the galvanometer spot takes a rapid swing in the direction denoting less balancing resistance required, but that it very soon stops and returns along the scale with great but diminishing velocity.

The above table shows the effect on the quantity of balancing resistance brought about by the selection as zero of any particular distance in time along the spot's path. Now, if, as was in this experiment observed, there is no appreciable electrostatic or electro-magnetic capacity in any of the arms of the bridge to disturb the galvanometer on opening or closing the battery circuit, the rush of the spot in the first direction must be due to the polarisation electro-motive force existing in the exposure circuit during the last moments of testing current flow, and consequently the correct position of the spot to select as zero is that position which in its first throw it endeavours to reach. Accordingly, in observing series No. 1, the balancing resistance was reduced step by step until this first throw was so far diminished as to be no longer appreciable. So rapid is this first throw that perhaps the vanishing limit was not actually attained, and the resistances may therefore be slightly in excess of their true values. This may account for the slight discrepancy between the observed and calculated coefficients already mentioned.

As further experimental proof of the law of inverse square roots, after eliminating the effects of polarisation, the following experiment may be added:—

#### *Experiment.*

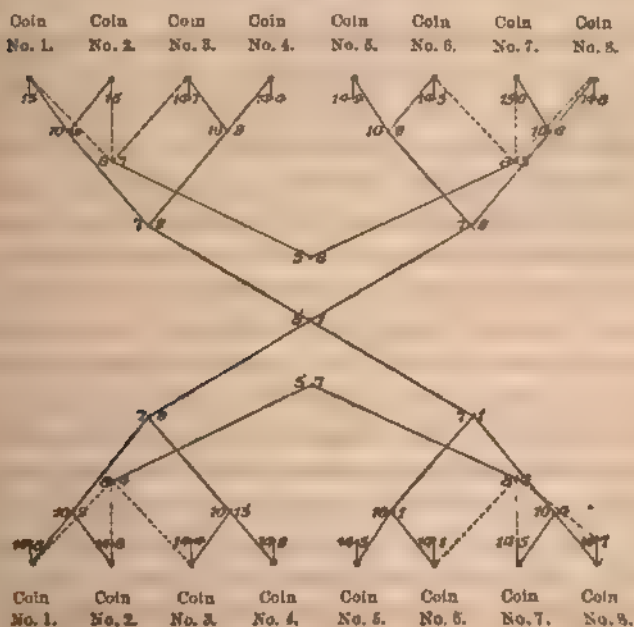
*Eight golden coins (sovereigns), mechanically cleansed, were*



immersed in sea water. By means of a single turn of fine insulated copper wire, bared at the end and wound round the milled edge of the coin, electrical communication was obtained. The resistances of the exposures formed by the surface of these coins were then measured to immediate false zero, in their different combinations, as in series No. 1 of the preceding table. The current in the exposure circuit was maintained approximately constant at 10 milliampères, the zinc pole of the battery being to bridge. The following table shows the resistances observed, the lines indicating the coins that were connected in multiple arc for each respective observation. It will be seen that the resistances fell slightly during the course of the experiment, which commenced with the reading at the upper left-hand side, and ended with the reading at the lower right-hand side of the table.

Table II.

COIN NUMBERS AND COMBINATIONS WITH CORRESPONDING RESISTANCES  
IN OHMS.



These observations may be classified as follow :—

					Ohms.
Mean resistance of exposures singly	from 16 readings				14.64
"	"	2 at a time	"	8	10.28
"	"	3	"	4	8.50
"	"	4	"	4	7.18
"	"	6	"	2	5.75
"	"	8	"	1	5.10

From 14.64, the mean resistance singly, and the law of inverse square roots we have—

					Ohms.
Mean resistance calculated of exposures 2 at a time	...				10.35
"	"	"	3	"	8.45
"	"	"	4	"	7.32
"	"	"	6	"	5.98
"	"	"	8	"	5.18

The discrepancies between the actually observed and the calculated results are thus all less than a quarter of an ohm, and if, the law being assumed, the mean readings are corrected to it instead of to the arithmetical mean, the discrepancies are still less.

The admission of the truth of this law for varying exposures and constant current makes, as we have already seen, the same admission necessary for the law with varying currents and a constant exposure, and hence the latter must hold good for all faults. But where the exposed surface is very small the products of electrolysis, not finding a clear path for free discharge, reduce the area of exposure in a variable manner, so that for such faults it is not to be expected that their resistances will obey the law. For suppose a constant difference of potentials to be maintained between the end of a cable that has a small fault and earth, the first current which flows through the exposure may be in accordance with the law. The accumulating and partially imprisoned gases or salts developed by electrolysis soon, however, reduce the actual area of liquid contact, and the resistance, we may suppose, rise proportionally to the square root of this reduction of area itself. This increased resistance will in its turn diminish the current, thus tending to alter the electrolytic develop-

ment, and so a continual readjustment and variation goes on in the observed resistance. When, however, the exposed surface is of such dimensions that the gases developed are readily disengaged, and do not accumulate, we may expect the observed resistances to fall within the law. This is, fortunately, the case in the majority of exposures that broken cables offer.

Turning now to the practical view of the subject, it may well be urged that it is impossible to arrive at the true resistance of the exposure with a broken cable by observing to immediate false zero, on account of the electrostatic discharge from the cable at the moment of interrupting the testing current; and since the apparent resistance of a fault measured to a false zero after one or more seconds' interval appears to follow a doubtful and complicated law, the knowledge of the actual law of inverse square roots, though scientifically interesting in itself, would lead to very little practical success in fault localisation.

Although the force of this objection cannot be denied, still it would appear in practice that for feeble currents—that is to say, testing currents below 25 milliamperes—the observed resistances do not seriously diverge from the law, except, perhaps, where the length of cable between the observer and the break is very short; and that, consequently, by observing the resistances with definite current-strengths the actual resistance of the break can be determined.

This is the more fortunate, since if the law were found to be followed by strong rather than by weak currents its efficacy would be limited to breaks comparatively close to the testing station. For instance, with a battery power whose electro-motive force is 50 volts and internal resistance 100 ohms, an observer using a Wheatstone's bridge with a ratio of  $100\omega : 1,000\omega$  can force 25 milliamperes through a break offering  $50\omega$  when it is  $1,740\omega$  distant from him; while with the same connections he could not force 200 milliamperes through the break further than  $30\omega$  from him, even if it only offered  $10\omega$  resistance at the time.

The following tables are copies of the results obtained with actual breaks, during the last six months, in the cables of the Eastern Telegraph Company, by whose permission they are placed

before the Society. In all these observations the law is submitted to a crucial examination, because the readings are here given just as they were obtained, and before the resistances of the various faults were at all accurately known. It was only in the last case, illustrated at Table VI., that the law was so far relied upon from previous experience as to assist the judgment in assigning the position of the break. In all but this last case the correct distances and resistances of the breaks were ascertained from tests on board the repairing ship that recovered the broken ends. All the readings were taken by bridge to a false zero on a rather dead-beat galvanometer as soon after the cessation of the battery current and the discharge from the cable as possible—generally between one and two seconds' interval.

Table III.

## BROKEN CABLE No. 1.

I. Number of Milliamperes in Testing Current passing through the Break. Zinc to Zinc.	II. Corresponding Resistance of Cable and Break observed.	III. Distance of Break as subsequently determined.	IV. Consequent Resistance of Break as observed.	V. Calculated Resistance of Break from No. 4 on law of inverse square roots.
	Ohms.	Ohms.	Ohms.	Ohms.
1	860	598.6	261.4	261.4
2	800	...	201.4	185.8
4	750	...	151.4	151.7
5	712	...	131.4	...
6	705	...	118.4	117.6
7	700	...	106.4	107.8
8	690	...	101.4	99.3
9	680	...	96.4	92.9
10	686	...	91.4	88.6
11	680	...	86.4	83.1
12	675	...	81.4	79.1
13	672	...	76.4	75.0
14	669	...	73.4	72.9
15	666.5	...	70.4	68.6
16	666.5	...	67.9	67.9
	666.5	...	66.4	66.7

The lowest resistance that could be obtained by Lumsden's method was 630  $\omega$  at 18.5 milliampères through the break, corre-

Table IV.

## BROKEN CABLE No. 2.

I. Strength of Test- ing Current. Number of Milli- ampères through Break, approximately. Zinc to Line.	II. Corresponding Resistance of Cable and Break observed.	III. Actual Distance of Break as subsequently discovered.	IV. Consequent Resistance of Break as observed.	V. Calculated Resist- ance of Break from first obser- vation with a milliampères by law of inverse square roots.
	Ohms.	Ohms.	Ohms.	Ohms.
4	280	144	86	...
5	320	...	76	76.9
6	215	...	71	70.2
7	210	...	66	65.0
8	205	...	61	62.2
9	200	...	56	57.3
10	196	...	52	54.4
11	193	...	49	51.9
12	190	...	46	49.7
14	180	...	44	46.0
15	187	...	43	44.4
16	186	...	42	43.0
18	183	...	39	40.5
20	181	...	37	38.4
22	178	...	34	34.4
30	176	...	32	31.4
35	175	...	31	29.1
40	174	...	30	27.2
45	173	...	29	25.6
50	171	...	27	24.3
55	170	...	26	23.2
60	169	...	25	22.2
65	168	...	24	21.3
70	167.5	...	23.5	20.5
80	166.5	...	22.5	19.2
90	165.5	...	21.5	18.1
100	165	...	21	17.2
110	163.5	...	19.5	14.5

sponding to what might have been expected from the above series at about 70 milliamperes. The allowance for the break then made was  $30 \omega$ , so that the estimated distance of the break was  $600 \omega$ . The close agreement between the figures of the two last columns is very striking, especially as the law was not known when the observations in column I. were made. In column V. the results are calculated from reading No. 4, the geometrical mean. The length of conductor exposed was found, on picking up the broken end, to be about  $\frac{1}{4}$  inch.

On the recovery of the broken end by the repairing ship, the length of conductor exposed was found to be 0.9 cms. The Lumsden minimum was  $157 \omega$  with 150 milliamperes.

By comparing columns IV. and V. in Table IV. it will be seen that for current-strengths greater than 25 milliamperes the observed resistances of the break begin to depart from the law, owing, no doubt, to the necessary loss of time in false-zero reading.

For instance, before 25 milliamperes are reached,		
The observed resistance of the break being	$86 \omega$ at	4 milliamperes,
ought by the law to fall to	$43 \omega$ at	16        "
and by column IV. apparently did so	at	15        "
Also, the resistance of the break being	$76 \omega$ at	5         "
ought by the law to fall to	$38 \omega$ at	20       "
and apparently did so	at	19       "

But after passing the range of 25 milliamperes,		
The resistance of the break being	$39 \omega$ at	18       "
ought by the law to fall to	$19.5 \omega$ at	72       "
but it apparently did not fall to	$19.5 \omega$ until	140     "

In the following case the result is not quite so accurate as in the two preceding. The cause of this may perhaps be accounted for later on. By the application of the law, however, to the resistances observed with currents below 25 milliamperes, the calculated distance of the break is only  $2.5 \omega$  from the true position. The Lumsden minimum was  $32 \omega$  at about 200 milliamperes.



Table V.

## BROKEN CABLE No. 3.

I.	II.	III.	IV.	V.
Testing Current through Break Milliampères. Zinc to Line.	Resistance ob- served, Cable and Break.	Distance of Break subse- quently known.	Consequent Resistance of Break observed.	Calculated Resist- ance of Break from first observation.
	Ohms.	Ohms.	Ohms.	Ohms.
5	71	22	49	...
10	60	...	38	34.7
15	55	...	33	28.3
20	50	...	28	24.5
25	46	...	24	21.9
30	43	...	21	20.0
35	41	...	19	18.5
40	40	...	18	17.4
45	39	...	17	16.3
50	38.5	...	16.5	15.5
55	38	...	16	14.8
60	37.5	...	15.5	14.2
70	36.5	...	14.5	13.1
80	35.7	...	13.7	12.3
90	35.1	...	13.1	11.7
100	34.8	...	12.8	10.9
120	34	...	12	10.0
140	33.2	...	11.3	9.2
160	33	...	11	8.7
180	32.6	...	10.6	8.1
200	32.2	...	10.2	7.7

Here again, before 25 milliampères,

The resistance of the break was observed to be 49  $\omega$  at 5 m-amps.,  
and by the law should fall to half, 24.5  $\omega$  at 20 „  
apparently doing so at 24 „

Beyond 25 milliampères,

The resistance of the break was observed to be 21  $\omega$  at 30 „  
and by the law should fall to 10.5  $\omega$  at 120 „  
apparently doing so at 180 „

Table VI.

## BROKEN CABLE No. 4.

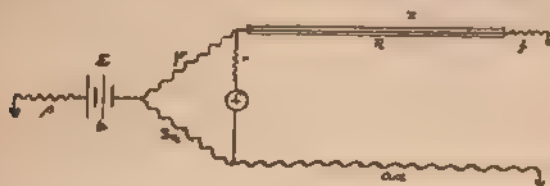
I.	II.	III.	IV.	V.
Testing Current. Milliamperes through Break. Kine to Line.	Observed Resist- ance of Cable and Break.	Estimated Distance of Break.	Consequent Resistance of Break observed.	Calculated Re- sistance of Break from observation with 4 milli- amperes.
	Ohms.	Ohms.	Ohms.	Ohms.
1	1,888.2	1,778	110.2	90.2
2	1,833.2	...	55.2	50.0
3	1,828.2	...	50.2	52.1
4	1,823.1	...	45.1	...
5	1,818.1	...	40.1	40.8
6	1,813.1	...	35.1	36.9
7	1,811.1	...	33.1	34.1
8	1,809.1	...	31.1	31.9
9	1,807.1	...	29.1	30.0
10	1,805.1	...	27.1	28.5
11	1,804.1	...	26.1	27.2
12	1,803.1	...	25.1	26.0
13	1,802	...	24	25.0
14	1,801	...	23	24.1
15	1,801	...	23	23.3
16	1,800	...	22	22.6
17	1,799.5	...	21.5	21.9
18	1,799	..	21	21.3
19	1,799	...	21	20.7
20	1,798.5	—	20.5	20.2
21	1,798.5	...	20.5	19.7
22	1,798	...	20	19.2
23	1,797.5	...	19.5	18.8
24	1,797	—	19	18.5

The actual distance of this break was not determined, but the estimated distance, 1,778  $\omega$ , must have been very near the true the Lamsden minimum being 1,783  $\omega$  at 27 milliamperes.

The preceding tables seem to show that when the resistance of a break is steady for a particular steady current traversing

of less than 25 milliampères, the resistances it offers follow the law of inverse square roots of the current-strengths with sufficient accuracy for the purposes of measurement. The question that next arises is how this measurement can be best effected.

Two methods suggest themselves—one by deflections on a galvanometer placed between the cable and battery, the said deflections being reproduced artificially; the other by Wheatstone's bridge. The bridge is perhaps the better means, because, although there is more trouble in arithmetically determining the proper testing current-strengths, still, with a dead-beat galvanometer, the false zero can generally be observed more quickly after the cessation of the testing current than in the deflection method. This, however, may be only a matter of opinion; but as all the observations in this paper have been by bridge, it will be better to here examine the bridge method only.



A cable of resistance  $R$  has its broken end offering a resistance  $f$ . The sum of these two—that is, the resistance observed with any given current—is represented by  $x$ . The arms of the bridge are  $\mu$  and  $a\mu$ ; so that  $a$  is generally unity, 10, or 100. The testing battery has an electro-motive force  $E$ , and an internal resistance  $b$ . Between it and earth an adjustable resistance ( $\rho$ ) is inserted, by which the strength of the current passing through  $f$  can be adjusted. It is better to add a resistance  $r$  to the circuit of the galvanometer if the latter be too sensitive, as, if balance be not attained, a shunt facilitates the increase or decrease of current to line beyond what is calculated for.

When balance is obtained, neglecting the electro-motive force due to earth current or polarisation in the cable, the strength of the current leaving the battery is

$$\frac{E}{\rho + b + \frac{a(\mu + x)}{a + 1}}$$

Of this current the proportion going to line is

$$\frac{\alpha}{\alpha + 1};$$

so that the current through the break is

$$\frac{E \alpha}{(\alpha + 1)(\rho + b) + \alpha(\mu + x)}$$

In order that this current may be exactly  $n$  milliamperes this expression must equal  $\frac{n}{1,000}$ ;

$$\text{from which } \rho = \frac{1,000 E \left( \frac{\alpha}{\alpha + 1} \right)}{n} - \left( b + \frac{\alpha \mu}{\alpha + 1} \right) - x \left( \frac{\alpha}{\alpha + 1} \right).$$

On the right-hand side of this equation  $x$  and  $n$  are the only variables; so that for a given battery and bridge ratio the numerator of the first term, the whole second term, and the fractional part of the third term are constants. When the arms of the bridge are equal  $\alpha = 1$ , and the equation becomes

$$\rho = \frac{500 E}{n} - \left( b + \frac{\mu}{2} \right) - \frac{x}{2}.$$

When the arms of the bridge are in the ratio of 10, with the lesser on the line side,  $\alpha = 10$ , and

$$\rho = \frac{909 E}{n} - \left( b + \frac{10 \mu}{11} \right) - \frac{10 x}{11}.$$

The *modus operandi* is therefore as follows:—

Measure  $E$  in volts and  $b$  in ohms.

Fix upon the bridge ratio to be adopted.

If 100  $\omega$  to 1,000  $\omega$  is preferred,  $\alpha = 10$  and  $\mu = 100$ .

Find  $909 \times E$ .

Divide this constant by the numbers of milliamperes that are to be forced through the break; that is, divide by 1, 2, 3, . . . successively, up to 25.

From all the quotients subtract the constant

$$\left( b + \frac{10 \mu}{11} \right) = (b + 91).$$

The differences remaining will be the values to give  $\rho$  in succession, after  $\frac{10 x}{11}$  (or, if  $x$  be not large,  $x$  itself) has been subtracted from them in turn.

Inasmuch as there will always be some polarisation, electromotive force, or earth current in the line, the actual current-strength through the break will not be exactly the amount calculated—generally less, in spite of reading to false zero. The best way to obviate this source of error, is to make  $E$  as large as is convenient. For instance, if  $E$  be 100 volts, the current-strengths through the break will probably be correct to 2 per cent. A correction is, however, given at Appendix II. One direction of current only must be adhered to throughout the series, and it is better to use the zinc pole to line, as the hydrogen so developed on the exposure, enters into no chemical combination, and if freely discharged keeps its surface clean and uniform.

When a series of resistances has been observed in this way, the deduction of the break's resistance is a simple matter.

It is best to take an odd number of the readings in the neighbourhood of 4 milliamperes, and the corresponding quadruples—say 3, 4, and 5, with 12, 16, and 20 milliamperes.

For instance, in the last table the resistance observed

at 3 milliamperes was  $1,828.2 \omega$

And the resistance observed at 12 milliamperes was  $1,803.1 \omega$

Now, by the law, the difference between these must be half the resistance the break offered at 3 milliamperes, and consequently the whole resistance it offers at 12 milliamperes.

Subtracting again, therefore, the distance of the break =  $1,778 \omega$

Again, the resistance at

4 milliamperes was  $1,823.1 \omega$  and at 5 milliamperes  $1,818.1 \omega$

Again, the resistance at

16 milliamperes was  $1,800 \omega$  „ 20 „  $1,798.5 \omega$

$\therefore$  the resistance of the

break at 16 mas. was  $23.1 \omega$  „ 20 „  $19.6 \omega$

$\therefore$  the distance of the

break =  $1,776.9 \omega$  and =  $1,778.9 \omega$

The mean distance of the break, from these three, =  $1,777.9 \omega$

From this method of calculation it follows that more care is required for the observation of the quadruple than of the first current. For if the error in observing the resistance with a current  $c$  be  $\alpha$  ohms, the error so produced in the result is  $\alpha$  ohms; but the same error,  $\alpha$ , in observing the resistance with  $4c$  will produce an error in the result, of  $2\alpha$ .

Calculated in this way,

Table V. from 5 and 10 milliamperes makes the break's distance  $24.5 \omega$  instead of  $22 \omega$ .

Table IV. from 4 and 5 milliamperes makes the break's distance  $142 \omega$  instead of  $144 \omega$ .

Table III. from 3 and 4 milliamperes makes the break's distance  $600 \omega$  instead of  $598.6 \omega$ .

The greatest error in the calculated distances of these three breaks would only have been  $2.5 \omega$ .

### Table VII.

To further illustrate the method, the complete operations for arriving at the results given in Table VI. may be admitted.

Battery, 38 Leclanché cells (52.344 volts and  $70 \omega$  internal resistance). Bridge,  $100 \omega : 1,000 \omega$ . Formula—

$$\begin{aligned} \rho &= 909.1 \times \frac{52.344}{n} - \left( 70 + \frac{10}{11} \times 100 \right) - \frac{10 x}{11} \\ &= \frac{47,585}{n} - 160 - \frac{10 x}{11}. \end{aligned}$$

Columns I., II., and III. are prepared before the test is taken. For the first few observations preliminary approximations to the value of  $\rho$  have generally to be made as the term  $\frac{10 x}{11}$  is not known; but after one or two readings, the rate of change in  $x$  is soon detected, and the right value of  $\rho$  can be inserted by inspection.

With the battery and bridge ratio employed it was evidently impossible to push the readings beyond 26 milliamperes.

The criterion of a good series is the closeness of its readings to those of a second or repetition series respectively.



I.	II.	III.	IV.	V.	VI.	VII.	VIII.
Current through Break. in am- pères. Due to Leakage	Constant (a), $\frac{47,585}{n}$	$a - 160$ . $\frac{47,585}{n} - 160$	First Approx- imation to $p$	Corre- sponding observa- tion of $x$ , corrected for temp. bridge.	$10x$ $\frac{11}{11}$	Second Approx- imation to $p$ . $\frac{47,585}{n} - 160 - \frac{10x}{11}$	Observed Value, $x$ corrected for temp. bridge.
			Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
1	47,585	47,425	46,000	1,900	1,726	45,700	1,888.2
2	23,792	23,632	21,900	1,850	1,682	21,950	1,833.2
3	15,862	15,702	14,000	1,828	1,662	14,040	1,828.2
4	11,896	11,736	...	...	1,657	10,080	1,823.1
5	9,517	9,357	...	...	1,653	7,700	1,818.1
6	7,931	7,771	...	...	1,647	6,120	1,813.1
7	6,798	6,638	...	...	1,646	4,990	1,811.1
8	5,948	5,788	...	...	1,645	4,140	1,809.1
9	5,287	5,127	...	...	1,643	3,480	1,807.1
10	4,769	4,609	...	...	1,642	2,960	1,805.1
11	4,326	4,166	...	...	1,640	2,530	1,804.1
12	3,966	3,806	...	...	1,639	2,170	1,803.1
13	3,660	3,500	...	...	1,638	1,860	1,802
14	3,399	3,239	...	...	1,637	1,600	1,801
15	3,172	3,012	...	...	1,637	1,375	1,801
16	2,974	2,814	...	...	1,636	1,180	1,800
17	2,799	2,639	...	...	1,636	1,000	1,799.5
18	2,643	2,483	...	...	1,636	847	1,799
19	2,504	2,344	...	...	1,636	708	1,799
20	2,379	2,219	...	...	1,635	584	1,798.5
21	2,266	2,106	...	...	1,635	471	1,798.5
22	2,163	2,003	...	...	1,634	369	1,798
23	2,069	1,909	...	...	1,634	275	1,797.5
24	1,982	1,822	...	...	1,634	188	1,797
25	1,903	1,743	...	...	1,634	109	1,797
26	1,830	1,670	...	...	1,633	37	...
27	1,762	1,602	...	...	1,633	...	...

For the future, we may well entertain the hope that the law may equally be found to apply to results obtained by the deflection method of measurement. If so, scarcely any calculation would be needed; for so long as the currents sent from the battery do not exceed 25 milliampères the resistances of the break would

be in proportion to the square roots, inversely, of the observed deflections from the true scale zero, while the artificially reproduced deflections would be adjusted to the polarisation or false zero only. It would therefore be merely necessary to adjust  $\rho$  so as to obtain convenient deflections from true zero and their quadruples, then observing the false zero as soon as possible in each case, to reproduce by resistance the deflection from that false zero. The absolute current-strengths through the break would not be of consequence.

It may also be possible to extend the range of current-strengths that we can employ by experimentally arriving at an empiric series of currents above 25 milliamperes for whose components, and for a particular interval of false-zero reading, the rate of diminution in the break resistance will be determinable.

When a break is stated to have a certain resistance, it is generally understood that the resistance named is the estimated minimum which Lumsden's treatment with the particular battery and connections employed, will produce. But if the exposure allows free discharge of gases, it is certain that under conditions which would force stronger currents through the break, this minimum would be still further reduced; and it is probable that this Lumsden's minimum, obtained at a time when the surface of the exposure is assumed to be free from gas or salts, corresponds to what would be obtained with a zinc to line series such as has been described, at the same current-strength, if it were possible to read to immediate false zero. Consequently, in order to convey an accurate or comparative idea of a break's resistance, it is necessary to mention the current-strength at which it was observed, and whether in a series, to a particular interval of false zero, or by Lumsden's method.

We may also hope, by collecting observations with broken cables, to predict for any given series observed, the length of conductor which will be found exposed. By experiments collected at Table IX. it appears that the resistance of one square centimètre of copper conductor to one milliampère, or what we may call more briefly the resistance per milliampère-square-centimètre, was about 70  $\omega$ . In Table II., the surface of a sovereign

being about 8.5 square centimètres, the resistance observed per milliampère-square-centimètre seems to have been about 130  $\omega$ . Owing to impurity of surface, however, and to delay in false-zero reading, the resistances of actual breaks referred to the unit area and current are greater. In the case represented by Table III. the exposure was observed to be about a quarter of an inch in length when the break was picked up, and in the case of Table IV. the exposure was found to be 0.9 centimètres long.

The core was in both of  $\left\{ \begin{array}{l} 120 \text{ lbs. copper} \\ 175 \text{ lbs. g. p.} \end{array} \right\}$  per knot; so that the exposed surface in No. III. would have been about 0.5 square centimètres, or from the observed readings 185  $\omega$  per milliampère-square-centimètre. In the case of No. IV. the surface would be about 0.7 square centimètres, and the observed resistance, calculated to the same unit, 145  $\omega$ . Selecting, in the absence of further data, the mean of these two as the resistance of the unit surface (165  $\omega$ ), the area exposed in the broken end referred to in Table VI. would be  $\left(\frac{165}{90}\right)^2$ , or 3.4 square centimètres; and in the above-mentioned core this would be represented by 4.5 centimètres of conductor, linear measurement.

Returning once more to the theoretical side of the question, two interesting points present themselves for investigation.

First. It appears from direct experiments, such as those shown at Table I., that for exposures artificially made, and with resistance but no electrostatic capacity in their circuits, the law of inverse square roots is only found to apply when the measurements are taken to immediate false zero. Even one second's delay seems to produce a large deviation. But in the case of actual exposures with broken cables the necessary delay of one second or more in observing the false zero does not apparently give rise to any great departure from the law. As one explanation for this very fortunate phenomenon, it may be suggested that the electrostatic discharge through the break upon the cessation of the testing current, may so far influence the electromotive force of polarisation as to keep it from falling at the rapid rate observed in experimental exposures on short circuit.

If this is the right explanation, it should follow that the error would tend to increase as the cable became shorter; and the case of Table V., which has more error than the others, and refers to a cable broken only two miles off, seems to bear the generalisation out.

Second. We know that when a metal plate is exposed in such a liquid as, for example, a solution of cupric sulphate, the resistance of the exposure is nearly constant for all currents. In fact, were it otherwise, it would follow that the internal resistance of a Daniell's cell would vary with the strength of the current it generated.

On the other hand, all the experiments above mentioned go to show that the same plate immersed in salt water offers a resistance at its surface varying as the square roots inversely, of the traversing currents, and thus apparently departing from what we might have expected by Ohm's law. The following experiment illustrates the matter:—

#### *Experiment.*

An exposure of about half an inch of No. 16 copper wire was immersed in sea water, and afterwards in a solution of copper sulphate. The resistances it offered to two series of currents, first with zinc and then with copper to line, are shown comparatively.

The measurement was made by bridge, with 30 Minotto cells and a ratio of  $\frac{100}{1000} \omega$ , in the manner described. All readings were to immediate false zero.

Table VIII.

I.  Current through Exposure. Milliam- pères.	II. III. IV. V. ½" Exposure in Sea Water.				VI. VII. In Solution Cu SO <sub>4</sub> .	
	Zinc to Line.		Copper to Line.		Zinc to Line.	Copper to Line.
	Resistance observed.	Calculated Resistance from No. 4.	Resistance observed.	Calculated Resistance from No. 4.		
	Ohms.	Ohms.	Ohms.	Ohms.		
1	97	98	85	27	For all these cur- rents the observed resistance was about 18·5 $\Omega$ , slightly un- steady above 20 milliampères.	
2	78	69·3	82	19·1		
3	55	50·7	11	15·6		
4	49	...	13·5	...		
5	42	48·8	11·9	12·1		
6	38	40	...	...		
7	36	37	—	...		
8	35	34·7	...	...		
9	33	33	...	...		
10	30	31	9·5	8·6		
11	28	29·5		...		
12	27	28·3		...		
13	25·5	27·2	Resistance, copper to line, suddenly increased above 10 milliampères.	...		
14	24·5	26·2		...		
15	24	26		...		
16	23·7	24·5		...		
20	21	21·9		...		
24	19	20		...		
30	16·7	17·9		...		
40	15·2	15·5		...		
50	13·5	13·9		...		
60	12·5	12·8		...		

The figures in columns II. and III. show how closely the resistances, with the zinc pole to bridge, follow the law of inverse square roots. With copper to line the law is not so well maintained, and broke off abruptly above 10 milliampères; but with the exposure in copper sulphate solution the resistance with both currents was the same for 60 as for 1 milliampère.

The explanation of this difference in behaviour may lie in the

fact that gas is developed on the surface of the plate in one case, and not in the other. The hypothesis may be advanced, in the absence of a better, that each bubble of gas, so long as it remains undischarged, undissolved, and not chemically reduced, may insulate the surface of the plate it covers; and that perhaps the rate at which this process of insulation disappears by the discharge of bubbles may be directly proportional to the square root of the quantities of gas developed in a given time, as these are known to follow the current-strengths directly.

But if these correlated laws of inverse squares connecting surface, resistance, and current recommend themselves to the Society on examination, and are corroborated by inquiry, it may well be anticipated that the true explanation of this interesting phenomenon will not long be forthcoming. Enough has been stated with the endeavour to show that besides the Lumsden method of estimating the resistance of a break, which will always remain in use, there is also an ample field open for measurement as a check upon judgment with the majority of breaks that present themselves in practice.

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## APPENDIX I.

### *Table IX.*

Table showing the resistances experimentally observed with different exposures of cable core in sea water, compared with the amounts calculated by the laws of inverse square roots from a single observation as basis.

The core selected was of  $\left\{ \begin{array}{l} 120 \text{ lbs. copper} \\ 175 \text{ lbs. g.-p.} \end{array} \right\}$  per knot.

30 Minottis = 34.49 volts }  
410  $\omega$  internal resistance } testing battery.

Bridge, 100  $\omega$  : 1,000  $\omega$ . Zinc to line. Immediate false zero.

To maintain the proportion in the surfaces, the section of the copper strand was capped in each experiment with insulating material. All the observed readings were steady.

Current through Exposure. Milliamperes.	Resistances observed.					Resistances calculated from Reading No. 4, column I. of observed readings, by laws inverse squares.				
	Approximate Length and Surface					of Conductor exposed in cms.				
	I.	II.	III.	IV.	V.	I.	II.	III.	IV.	V.
	Cms.	Cms.	Cms.	Cms.	Cms.	Cms.	Cms.	Cms.	Cms.	Cms.
	8	4	2	1	0·5	8	4	2	1	0·5
	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.	Sq. cms.
	5·872	2·336	1·468	0·734	0·367	5·872	2·336	1·468	0·734	0·367
	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
1	26	38	50	72	102	28	39·6	56	79·2	112
2	18	28	35·5	55	70·5	19·8	28	39·6	56	79·2
3	16	22·5	32	45	61	16·2	22·9	32·4	45·8	64·8
4	14	20	27·5	38	54	...	19·8	28	39·6	56
5	12·7	18·5	25·5	36·5	48·5	12·5	17·7	25	35·4	50
6	12	17	24·5	33·5	45	11·5	16·2	22·9	32·4	45·8
8	10	14·5	19·5	28	37	9·9	14	19·8	28	39·6
12	8·5	12	17	23	33·7	8·1	11·5	16·2	22·9	32·4
16	7·5	10·5	14·7	20	28	7	9·9	14·0	19·8	28
20	6·5	9·7	12·9	18·4	24·6	6·3	8·9	12·5	17·8	25
24	6·2	9·0	12·3	17	23	5·7	8·1	11·5	16·2	22·9

The discrepancies in the above table between the resistances observed and calculated, nowhere exceed 12 per cent., but the observed readings include errors of electrical measurement and of surface measurement, besides the second error of polarisation mentioned in the paper; *i.e.*, the stronger currents are probably overstated, thus making the resistance observed at those currents too high.

## APPENDIX II.

### THE ELIMINATION OF THE SECOND POLARISATION ERROR.

The correction for the alteration in the strength of the testing current to line owing to the earth current or polarisation electromotive force acting in it could be arrived at, when necessary, by observing a few of the readings to true zero as well as to false zero.



For if  $x$  is the resistance which at any given current-strength balances to true zero, then  $y$  is the apparent resistance of the line and break to true zero.

Referring to the figure, if  $\pm e$  be the electro-motive force owing to earth current or polarisation in the line, it can be proved that

$$e = \frac{(x - y) E}{\mu + y + \frac{(\rho + b)(a + 1)}{a}}$$

Thus, when  $y$  is greater than  $x$ ,  $e$  is negative, or acts against the testing current.

Owing to loss of time in false zero reading  $x$  is generally greater than its true amount, so that  $e$  is generally too small. When  $a$  is 10 or 100, the actual strength of current passing through the break when this value of  $x$  was observed would be approximately,

$$\text{Calculated current} \times \frac{E + e}{E}.$$

So that by observing  $x$  and  $y$  together at one or two points in the series the numerical correction to the current-strengths can be subsequently applied.

#### APPENDIX (*November, 1886*).

The results with broken cables 5 and 6 have been obtained since the above paper was written.

In the case of No. 6 the full correction of the current strength passing through the break is added, taking the electro-motive force of polarisation set up by the break and earth current into account. The comparison of columns I. and XVI. shows how slight the correction becomes with a battery of considerable strength. A series is also shown with the copper pole to line. This broke off abruptly on reaching 9 milliamperes, the resistance of the break rising suddenly from 42  $\omega$  to 67  $\omega$ , and increasing. The readings in this series are lower than with the zinc current, but not so uniform.

For practical ship's use the resistance  $\rho$  may be conveniently





calculated for a given battery, and checked from day to day. The values of  $\rho$  for 3, 4, 5, 6, and 12, 16, 20, and 24 milliampères being thus kept ready for use, four pairs of quadruple readings can generally be obtained with any break that may present itself in ten or fifteen minutes, the distance of the break for any given pair being then almost evident from inspection.

### BROKEN CABLE No. 5.

Zinc pole to line. 38.75 volts, 85  $\omega$  internal resistance battery.  
Bridge ratio,  $\frac{1000}{100}$ .

Milliampères through Break.	Resistance observed, corrected for temp.	Distance of Break assumed.	Resistance of Break observed.	Calculated Resistance of Break.
	Ohms.	Ohms.	Ohms.	Ohms.
1	1,700	1,532	188	156.5
2	1,645	...	113	113.7
3	1,620.5	...	88.5	90.4
4	1,610.25	...	78.25	...
5	1,600.5	...	68.5	69.9
6	1,595.5	...	63.5	63.9
7	1,590.5	...	58.5	59.2
8	1,587.5	...	55.5	55.3
9	1,585	...	53	52.2
10	1,582.5	...	50.5	49.5
11	1,580.5	...	48.5	47.2
12	1,578	—	46	45.2
13	1,576	...	44	43.4
14	1,573.5	...	41.5	41.8
15	1,571.5	...	39.5	40.4
16	1,570.5	...	38.5	39.1
17	1,569.5	—	37.5	38
18	1,568.5	...	36.5	36.9
19	1,567.5	...	35.5	35.9
20	1,567.5	...	35.5	35
24	1,565.5	...	33.5	31.9
25	1,564.5	...	32.5	31.3

The  
President.

The PRESIDENT: It is to be remarked that the practical applications set forth in Mr. Kennelly's paper are generally limited to tests when large exposures occur, where in the majority of cases the cables are totally broken; and so our attention regarding the paper should be principally directed to total breaks. A considerable percentage of the faults in submarine cables are not of this character, but are small faults in which the copper is only exposed to a very slight extent. It would be most desirable to be able to separate the resistance of the fault in such cases from the resistance of the line; but I am afraid that it will be very difficult to find an accurate formula for this, because the condition of such faults, from polarisation and other causes, is so extremely variable that the false zero can hardly be obtained; and Mr. Kennelly himself states that his experience is that the third interference is "electrically, because this electro-motive force of polarisation existing in the circuit whose resistance is to be measured causes a large error to be made in the measurement, and, owing to the great rapidity with which it falls, the moment the testing current ceases, the exact correction to apply is difficult to obtain." I have found the same thing myself; but I would rather now invite any of the members present who have had experience in cable repairs to give their views as deduced therefrom.

Sir Henry  
Mance.

SIR HENRY MANCE: As Engineer in charge of the Persian Gulf cables, the resistance of faults has been a subject which of necessity has occupied my very serious attention for many years. Our President has mentioned that his experience has been more in connection with faults which occur in the factory during manufacture—my own, on the other hand, has been confined to the maintenance of cables which, as it happens, were originally laid by Sir Charles Bright in 1864, and which, as may naturally be expected, gave plenty of opportunities for fault-testing towards the latter part of their existence.

In May, 1884, I contributed to this Society a paper on fault-testing, showing how, by a very simple method, the errors due to polarisation and earth currents might be eliminated. I thought *then, and I think now*, that the method was a distinct advance in

the course of testing usually followed. I was not present when that paper was very kindly read for me by Mr. Latimer Clark, and I must confess to a feeling of surprise and regret when I found that the paper had been criticised in an unfavourable manner by a member whose opinions usually carry with them considerable weight. It was argued that every electrician found out for himself how to allow for the errors that faults are likely to introduce. I admit that electricians of the old school, some of whom were men of exceptional skill and experience, did deal successfully with faults without the assistance of exact formulæ, but this required an amount of judgment not possessed by all of us; and every electrician who has had any practical experience in testing faulty cables will, I think, honestly confess that, when testing to a broken end, he could never be certain to a mile or two. You were told on that occasion by Mr. Preece that it was merely another case of History repeating itself. Well, History does repeat itself sometimes, but at the same time it generally brings us new facts and new experiences: for instance, I am sure we have been taught something fresh, something useful to-night; indeed the author has left us but little to learn in connection with faults. He has given us exactly the information we required. He has supplied the missing link, so that, when the time comes for History to repeat itself again, I don't think another paper on the resistance of broken ends will be necessary.

Sir Henry  
Mance.

Mr. Preece stated that the errors due to polarisation and earth currents were fully dealt with by Culley, and with this remark I would respectfully beg to differ. Lumsden's method is described, but this test requires considerable dexterity: it is frequently only approximate in its results, for it makes no allowance for earth currents, which might introduce most serious errors. When there are faults on the line capable of being polarised, you cannot eliminate the effects of earth currents by testing with alternate positive and negative currents: reversals of current will not assist us, as instead of one observation being higher and the other lower than the true value, as would be the case if earth currents only were disturbing the test, the tendency of the polarisation current is to make both observations much

His Henry  
Mance.

higher than the true value. Mr. Culley gives us no exact method: he gives a plan of eliminating the effects of polarisation by applying a negative current for 10 or 12 hours; whereas by the method given by me, the errors due to earth or polarisation currents can be ascertained by a simple test which should not occupy two minutes.

With your permission I will put upon the board the formulæ given by me in the paper to which I have referred—

$$\frac{R_1 (2r + P_1) - R_2 (2r + P_1)}{R_2 + P_2 - R_1 - P_1}$$

and the method of testing was as follows:—

Using the ordinary Wheatstone bridge with equal proportions, and testing with a continuous negative current, you obtained two observations, using during the first (say) the hundred to hundred proportion coils, and during the second the thousand to thousand coils.

$R_1$  = the resistance observed with the smaller proportion coils.

$R_2$  =                   "                   "                   larger                   "                   "

$P_1$  = the resistance of *one* of the smaller proportion coils.

$P_2$  =                   "                   "                   larger                   "                   "

$r$  = the internal resistance of testing battery.

Of course the test may be taken with unequal coils, but then the formula is not quite the same; or, if you prefer it, the  $\frac{1}{3}$  and  $\frac{1000}{1000}$  coils may be used, or equal coils of any other value. I mention the hundreds and thousands because they are provided for us in the testing bridge.

If when testing by this method the two readings are the same, no correction is necessary—the earth current has exactly balanced the current arising from polarisation. This, however, rarely occurs. As a general rule, the observed resistance with the smaller pair of proportion coils is lower than that obtained with the higher, the corrected result will therefore be less than either. Should a strong positive earth current be coming from the cable, it is possible that the  $\frac{1}{3}$  reading will be greater than that obtained with the  $\frac{1000}{1000}$  coils, in which case the corrected or true resistance will be greater than either of the observed results.

*I wish to observe that in my paper I claimed only to eliminate*



the effects of polarisation and earth currents, not to ascertain the exact resistance of the fault itself; but I pointed out that when the tests, which, without any alteration in the fault, sometimes vary considerably, are stripped of the errors due to these causes, you are in a position to form, by the regularity of your corrected results, a very accurate idea of the actual resistance of the broken end. Assuming the tests to be steady, with no disposition in the fault to increase in resistance under the influence of a positive current, you are quite safe in allowing about 5 ohms for a broken end close to the cable ship, or double that resistance if it happens to be a hundred miles distant. When that distance was greatly exceeded, I found the resistance of the fault began to be a serious matter, requiring a certain amount of judgment; but with the law the author has now given us, there appears to be no reason why the resistance due to the fault itself should not be accurately ascertained, even when the broken end is several thousand ohms away.

Sir Henry  
Mance

In consequence of my absence I have only been able to give a short time to the perusal of this paper; I cannot therefore do full justice to it during this discussion. I promise myself much pleasure in following Mr. Kennelly in the experiments he has made. If I understand him aright, the law he has given us is—if you quadruple the strength of the current passing through the fault, you halve its resistance. As I travelled up to London this afternoon, I referred to the table of resistances given with my paper, and it seems to corroborate the author's theory. I do not attach importance to the trifling discrepancies in the results given: the latter are sufficiently uniform for all practical purposes.

The testing instructions look a little complicated. We should remember that the majority of the clerks at submarine cable stations are not very skilful observers; as a general rule, they have few opportunities for practice. It is occasionally very convenient to be able to rely on tests taken by comparatively inexperienced testers; the simpler, therefore, you can make the test the better. The false zero method is open to the objection that, as polarisation currents are set up immediately, the cable zero is altered to some extent directly the testing current is

Mr Henry  
Mance.

applied. The rush of current into a cable on first applying a battery also affects the accuracy of the method, which consequently requires the exercise of considerable skill on the part of the observer.

I have spoken rather fully about my own method, because it appears to me that all we require to enable us to apply Mr. Kennelly's law is two corrected results taken in the manner I have described, the one pair of readings being obtained with a battery current four times as strong as the other. The difference between the corrected results should then give the resistance due to the fault itself during the test taken with the stronger battery power.

I cannot conclude without remarking that the author appears to have provided us with a law which will be of immense assistance to electricians engaged on cable work, especially when the break is a considerable distance from the testing station.

The  
President.

The PRESIDENT: I might remind Sir Henry Mance of an occurrence during the laying of the Persian Gulf cable in 1864, to which he referred, and that was that we had a fault before we had actually completed the line, which broke down between Guadur and Kurrachee. Mr. Laws, who, as many of us remember, was a very skilled electrician, was with me at the upper end of the Gulf. I sent him off in one of our cable fleet; he took a test from Guadur and struck the spot, where the cable was soon repaired by Mr. F. C. Webb within a quarter of a mile of the place indicated; and that, I think, speaks something for the knowledge and experience of some of the old electricians and cable engineers.

Mr. Ansell.

Mr. HAROLD W. ANSELL: I would like to mention one or two instances in which Mr. Kennelly's method has proved invaluable. On one occasion when the repairing steamer was paying out towards a buoyed end, on arriving at the buoy it was found that the cable had parted, and the question was—where? The test was required to be quickly and accurately taken—quickly, because the cable was weak and the ship was lifting to a head swell; and accurately, as a rough bridge test put the fault in or close to deep water. A series was taken—I think four couples—which occupied *ten minutes*, and the distance given was 35 miles, or just in the

shoal water. On recovering the fault some hours afterwards, the tests proved to be less than an ohm out, and not a "quarter of a mile."

The PRESIDENT: Was that an actual break?

The President.

Mr. HAROLD ANSELL: Yes; an actual break. It was on a rocky bottom, and the copper had chafed off close; very little was exposed, for it was nearly covered in by the gutta percha.

Mr. Ansell.

Another instance was where the cable was broken on the opposite shore to which we were testing, and the core lying on the wet sand; there were 32 miles of cable in circuit. The whole series was taken, zinc to line, with 40 Leclanché cells, and the results in this case were between 3 and 4 ohms out, the greatest discrepancy that ever occurred in these tests. The series occupied half an hour. The mean second swing of the galvanometer was balanced to earth current zero, that being the best balance for the discharge from the cable. It is a matter of experience as to the allowance for this in making these tests, but perhaps the plan works better upside down, taking the greater number of milliamperes to line first, then the corresponding couple, such as 24-6, 16-4, and so on, instead of going right through the series, 1, 2, 3, 4, 5, etc.

In regard to Sir Henry Mance's paper, which I have read several times and applied, I never could find that method work. Perhaps, however, in justice to the paper, I might say that I had not the form of bridge which is described.

Sir HENRY MANCE: This is not absolutely necessary.

Sir Henry Mance.

Mr. HAROLD ANSELL: The zero always shifted.

Mr. Ansell.

Sir HENRY MANCE: You were wrong in taking the test by the false zero method.

Sir Henry Mance.

Mr. HAROLD ANSELL: I don't mean that. It does not seem to work: only once were the results anything like correct.

Mr. Ansell.

I can only say that Mr. Kennelly has given us a test which is, I might say, perfect. The ordinary Lumsden method, before, always left something to desire, and *something* (how much was gained by experience) always had to be allowed, 5, 10, or some times 70 ohms. It was not at all satisfactory; but now nothing is required to be allowed on the test at all,—you get your figures

Mr. Ansell. right in front of you—you go to the spot and find the fault exactly.

Mr. Granville.

Mr. W. P. GRANVILLE: Would it not be possible to combine Sir Henry Mance's method of eliminating the polarisation currents with Mr. Kennelly's test for determining the resistance of a fault? In both cases it is necessary to obtain two measurements of the total resistance of cable and fault, the two values being obtained in Sir Henry Mance's test by altering the resistances in the arms of the bridge, viz., from 1000 : 1000 to 100 : 100; and, in Mr. Kennelly's method, by varying the battery so as to alter the strength of the current flowing through the fault.

It is evident that altering the resistances in the arms of the bridge means altering the current flowing through the fault, and therefore it appears to me that, by placing a galvanometer in the cable branch, so as to measure the current, we could first eliminate the polarisation effect by means of the formula on the blackboard, and then, by Mr. Kennelly's method, ascertain the resistance of the fault.

Mr. Donovan.

Mr. H. C. DONOVAN: I am very much pleased with Mr. Kennelly's paper, but I should like him to have touched upon the fact that the resistance of a fault is greater or less according to its surroundings. For instance, I have noticed that if the end is hanging in clear water, then there is a very large and varying resistance; but if it is touching mud, the resistance is low. With regard to Sir Henry Mance's paper, I do not think it was criticised in the way that he mentioned. Attention was drawn to the belief that Sir Henry Mance had a wrong idea of a wiping-out current, and many speakers thought that it was not necessary, but that we should work to the cable, or false zero.

The President.

The PRESIDENT: I do not understand how you are to know what is going on at the bottom of the sea, to assist you in calculating the varying resistance of the end that you speak of.

Mr. Donovan.

Mr. H. C. DONOVAN: It often happens that we are able to form an approximate idea, by known soundings, of what the nature of the bottom is, and where the end is likely to be in mud a low resistance should be expected; but if it is in deep water, and very

likely dangling over a rock, it may then be assumed that the resistance of the fault is greater than it would be otherwise.

Mr.  
Donovan.

Professor W. E. AYRTON: I have listened with the greatest interest to what Sir Henry Mance has said this evening, because on the occasion to which he refers I happened to be one of the few who saw that there was something of real value in his paper, and I did my best to warmly support it. I am afraid there is much truth in what he has just said regarding the way in which that paper was received, and rather pooh-poohed; in fact it was implied that in this country, where many of us had not much to do with submarine cables, we knew all about the matter, and that there was no information for those who were engaged with submarine telegraphy in the East to give us about faults. Of course it is very easy here, sitting in our arm-chairs, to criticise what the hard workers do on the other side of the world in those very hot and wild countries; but as I happen to have been in the hot and wild countries, my sympathy is naturally with the workers there. Therefore I must say that on that occasion I did my best to support what was told us in the paper of Sir Henry Mance, as he himself has on this occasion done to support what Mr. Kennelly has told us in his paper.

Professor  
Ayrton.

The question asked by Mr. Granville is really a very important one, and this question reminds me that, while Mr. Granville himself understands the matter, there may be very likely others here who are not quite clear as to the exact object of making each of the tests that we have heard described to-night. Of course the main object is to find the position of the fault; but what are the exact conditions that we have in a telegraph or submarine cable line? There is the resistance of the line up to the fault, say  $l$ ; there is the resistance of the fault, say  $f$ ; and there is the electro-motive force in the fault, say  $e$ . What we want to do is first to find  $l+f$  independently of  $e$ , and then  $l$  independently of  $f$ . Previous to Sir Henry Mance's paper, the first operation was done by reversing the testing battery; but Sir Henry Mance pointed out that that was not necessary, and that the result could be obtained by altering the value of the arms in the bridge. That really is a means of altering the current flowing



Professor  
Ayrton.

through the line; reversing the battery is another means; there are many ways (and I think I suggested some of them at the time) by means of which the current can be altered. From the two tests made with different currents flowing, it is then possible to separate the effect of resistance from the effect of electro-motive force. That, however, was obviously not clear to the world generally, because some time after that we had a paper read before this Society to show how it was possible to separate these two by only making one test. We had a long description of an impossible kind of experiment to show how with one equation containing  $x$  and  $y$  it was possible to find both  $x$  and  $y$ ! That, I think, was after Sir Henry Mance's paper was criticised as containing no novelty.

Of course it is impossible to determine two unknown things by *one* test: *two* tests are necessary. The old way of proceeding was to reverse the testing battery, and so alter the current passing along the line. Sir Henry Mance's way is to alter the current passing along the line by altering the arms of the bridge, and he only alters the current by a small amount; for the smaller the amount by which the current is altered the more accurate the test, since then the electro-motive force will be altered very little. We thus obtain two values from which the real resistance can be calculated independently of the electro-motive force in the line or fault. In those methods it is assumed that the electro-motive force is constant, and our effort is to avoid changing the electro-motive force in the line or fault, or changing what may be called the simple earth current. Now Mr. Kennelly has taken up the subject in a totally different way. He says we will not try to separate  $l+f$  from  $e$  by making two ordinary tests, but we will use the false zero, which by itself is in reality making one test. You observe the deflecting position of the needle, and that is a test by itself; next you alter the resistances in the bridge so as to obtain balance, or rather so as to reproduce the deflection, and this is the second test. Mr. Kennelly's next object is to enable us to separate  $l$  from  $f$  with a broken line. I do not remember whether Sir Henry Mance's tests referred to a *total break*.

Sir HENRY MANCE: Yes; a total break.

Sir Henry  
Mance,  
Professor  
Ayrton.

Professor AYRTON: At any rate, Mr. Kennelly's paper refers solely to a total break, and his next object is to separate  $l$  from  $f$ . The method that has hitherto been employed for this purpose with a broken cable is to combine the Wheatstone bridge test with the discharge test. Mr. Kennelly's method consists in finding out how  $f$  varies, by varying the current. We have an  $l + f$  known; one portion of this can be made to vary by varying the current, the other portion of which can not be so made to vary.

He has experimentally proved the law that  $f = \frac{f_0}{\sqrt{A C}}$ , where  $A$  is the area of the fault, and  $C$  is the current passing through the fault, and where  $f_0$  would be the resistance of the fault per unit area and unit current passing through it. Now, obviously, on making two or more tests, he knows that one part only of his result is varied, and so he is able by these tests to separate  $f$  from  $l$  in a way which appears to me extremely ingenious. Although Mr. Kennelly referred to some experiments made by my colleague and myself on this subject, I did not, I must say, know that the resistance of the fault was inversely as the square root of the current. Like Sir Henry Mance, I have just referred to our paper of 1878, which Mr. Kennelly mentioned, and I see that some of our results in that paper do certainly bear out his law, but other results that were given in the paper referred to do not seem to bear out the law. The establishment, however, of this very simple law seems to me to be making great strides, in this subject of determining the position of the fault, over the old method. There is one question I should like to ask, but the author of the paper is unfortunately not present,—it seems to be a rule that authors of excellent papers on fault-testing should not be present, they are absent in distant countries,—but somebody can no doubt answer this question about a point on which I am not quite clear. I am not clear from Mr. Kennelly's paper as to the exact way in which he makes his tests. I fancy what he does is this—I may be wrong, but perhaps somebody will correct me:—he has a Wheatstone bridge to line with the fault, consisting of proportional coils attached on one side, and on the other a resistance put to



Professor  
Ayrton.

earth, and by altering the resistance, called  $p$  in his paper, in the battery circuit, he makes a certain current pass through the line. He next has to test for the resistance, and, if I understand rightly, he does this by disconnecting the battery and measuring the resistance by using as his electro-motive force the electro-motive force in the fault (in fact, he uses that well-known method, Mance's method, of finding the resistance of a battery by using the electro-motive force of the battery—a method consisting in putting the battery in one arm of a bridge, and finding out what is the resistance of that battery, without using any testing battery). I do not know whether I am right in assuming that Mr. Kennelly uses this plan; if so, he must have another wire which he puts to earth, and so completes the circuit through another, this other wire having no electro-motive force, and which is put in place of the battery circuit.

Mr. Ansell.

MR. HAROLD ANSELL: The internal resistance of the battery is taken first: the battery is kept on until you get the maximum steady reading, and then it is released and the balance is obtained.

Professor  
Ayrton.

PROFESSOR AYRTON: The balance is obtained after removing the battery?

Mr. Ansell.

MR. HAROLD ANSELL (advancing to the board): Here is the scale zero [drawing], and you adjust your false zero to that zero [describing in detail].

Professor  
Ayrton.

PROFESSOR AYRTON: Thank you; I understand it better.

Sir Henry  
Mance.

SIR HENRY MANCE: I would remark that I have attempted to measure the resistance of a broken end by treating it as a cell, and using the method for ascertaining the internal resistance of batteries, which is given at page 411, vol. i., Clerk Maxwell; but when testing a fault by means of its own current, which is frequently extremely feeble, I found that a broken end which only offered a resistance of 10 or 12 ohms, when tested in the ordinary way, appeared to increase in resistance enormously,—the difference was so great that I was under the impression some mistake had been made,—but the law that Mr. Kennelly has given us to-night shows that, as a matter of course, when using such a feeble current the resistance of the fault would be something very high.

The PRESIDENT: You also found, I presume, that the current would fall very rapidly. The President.

Sir HENRY MANCE: There is sure to be some current from the fault. I do not remember that I applied the testing battery. I experimented with the weak current set up in the defect itself, and got by my battery method very high results which I did not understand at the time. Sir Henry Mance.

Professor W. G. ADAMS: Professor Ayrton has stated what are the three points which have to be determined in order to discover the position and resistance of a fault in a cable, viz., the resistance of the line up to the fault, the resistance of the fault itself, and the electro-motive force in the fault arising from the exposure of the conductor. In testing by means of a battery with the zinc pole attached to the line, the fault has a resistance which will vary with the amount of the deposit accumulated upon it, and the electro-motive force at the fault will be opposed to the current. If now the testing battery be removed, and the tests be made by means of the polarisation current from the fault, the resistance of the fault, as well as its electro-motive force, will be totally different in the second case, when the current is flowing in the reverse direction through the line. The theory of the matter would lead us to expect the best results from the method of measuring which has been advocated by Sir Henry Mance, viz., by keeping the zinc pole of the battery attached to the line, and altering the resistance in the branches of the Wheatstone bridge leading to the cable, and at the same time measuring the current flowing through the cable by means of a galvanometer. Professor Adams.

Mr. RYMER-JONES: I have taken great interest in the subject of fault-testing, and I must say that what Sir Henry Mance complains of, with regard to the want of interest shown in contributions to this subject, is well founded. Mr Rymer-Jones.

I remember quite well that, at the time when he put his formula before us, it was rather imagined by some that, although very good, the test was scarcely required, as the same thing, it was implied, could be done by other methods.

The discussion which has taken place to-night merely touches

Mr. Rymer-Jones. upon fault-testing, viz., those of very low resistance, and which are therefore comparatively easy to localise.

We hear so very little upon this subject that all the encouragement possible should be given to those who will come forward and elucidate a most difficult subject.

Mr. Kennelly's paper seems to be very valuable in filling up a serious gap, and, by showing how the resistance of the fault itself may be calculated, he amplifies the method of Sir Henry Mance, which effectually eliminates the error due to polarisation set up in the fault, and the influence of cable currents, and thus overcomes the greatest difficulty in faults of low resistance, though it does not give the resistance of the fault separately.

The only weak point that I can see in Mr. Kennelly's test is that he finds the resistance of the fault to vary inversely as the square root of the current passing through it only when the current strength is *below* 25 milliampères, and beyond that the same law does not apply. That seems to me to be a very weak testing current, and would probably in some cases, and more especially in long cables, be very much affected by cable currents.

I am very pleased to hear that such good results have been obtained by Mr. Kennelly's method, which is new to me, and I shall take the first opportunity of trying it myself.

The instance of its application mentioned just now to a fault 35 miles distant does not, however, give one an idea of what would be got on a long cable, because there would not, as a rule, be much earth current in so short a length as 35 miles; but in longer cables it sometimes varies considerably, and the false zero would vary also.

In the Mance tests, which I have used several times with very good results, a large battery can be employed, so that the cable current represents only a small percentage of the current going through the fault, and has less influence on the result.

As far as I can judge of the test, I shall feel inclined to continue to use Sir Henry Mance's formula, which has given me excellent results, to get rid of the effects of polarisation; and, by repeating the test with different strengths of current, obtain the *necessary* data to apply the correction given by Mr. Kennelly's

table for the number of milliamperes passing through the fault during the test, in order to ascertain its resistance. Mr. Hymer.  
Jones.

Sir DAVID SALOMONS: I really know nothing about cables, except from holding samples in my hand, and from reading of them in books. I therefore came to-night, not to speak on the subject, but to seek for information. As far as we have gone there are two or three points that I should very much like, before leaving this room, to ascertain, viz., how far the new law dealt with in this paper is true or not true. No doubt my questions may be answered by Sir Henry Mance, and perhaps by Professor Ayrton. I followed the paper very closely, and, as far as I see, the law is established from a series of experimental tests. Am I right in that conclusion? The law has been simply obtained from a series of tests which have been afterwards put in mathematical form, and only applicable, as far as one can see, to faults in sea water? So I judge from the special remarks which the author makes towards the end of his paper. If the law is true, it is certainly a very great advance, and apparently no one knew of it before; but I should like to know whether this law can be mathematically demonstrated, as is usually the sequence to the discovery of a new law by experiment. Sir David  
Salomons.

Professor W. E. AYRTON: I think it is like any chemical experiment: you cannot demonstrate it mathematically. Professor  
Ayrton.

Sir DAVID SALOMONS: It is demonstrated afterwards. I think our friend Professor Crookes can show us a good deal in that direction. Sir David  
Salomons.

Professor AYRTON: The mathematics are based upon the experiment. Experiment shows a certain thing, and mathematics demonstrate it. Professor  
Ayrton.

Professor PERRY: I have no doubt that at the end of a year or two somebody will say that it is all in Clerk Maxwell! Professor  
Perry.

The PRESIDENT: Our discussion is getting a little desultory, and the usual time has arrived for closing the meeting. The  
President.

I feel quite sure, from the interesting discussion which has taken place with regard to Mr. Kennelly's paper, that you will agree with me that we should accord him our hearty thanks for the paper in which he has taken so much pains to elucidate the

the  
resident.

experiments he has carried out in practice. The motion was carried by acclamation.

A ballot took place at which the following were elected :—

*Associates.*

Francis C. Crawford.	William Llewellyn Preece.
John Gray, B.Sc., A.R.S.M.	Arthur Molyneux Sillar.
Luther Hanson.	W. E. Sumpner.
Charles Edward Lowndes.	

*Students.*

Alfred George Bessemer.	George H. Hume.
James Mountjoy Elliott.	Arthur Henry Preece.
Conrad Krauss Falkenstein.	

The meeting then adjourned until 28th April.

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# ORIGINAL COMMUNICATION.

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## THE LIMITING DISTANCE OF SPEECH BY TELEPHONE.\*

The following table was accidentally omitted from the last number of this Journal. It summarises a large number of experiments that have been made in different parts of the country, and on different lines, to determine by observation the connection that exists between speed of current, distance spoken through, resistance, and capacity.

It will be seen that the limiting distance through which it is possible to speak varies inversely with the speed of the current, and that the speed of the current varies inversely with the product of the total resistance and the total capacity of the circuit. Hence we can say that the number of reversals that it is possible to send through any circuit varies inversely with the product of the total resistance ( $R$ ) and the total capacity ( $K$ ), or the limiting distance

$$S = KR \times \text{constant.} \quad . . . . . (1)$$

This is only another form of Thomson's law for  $K = lk$ , and  $R = lr$ , and

$$\therefore S = krl^2 \times \text{constant.}$$

It is seen that when the speed of the working current was

0.001" speaking was perfect,  
0.002" speaking was good,  
0.003" speaking was fair,  
0.004" speaking was difficult.

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\* This communication forms the appendix to Mr. Preece's remarks on Prof. Thompson's Paper (see *Society's Journal*, page 85).

Wire.	Route.	Length in miles.	Total resistance $\times$ capacity.	Speed of current in seconds.	Telephonic notes.
Nos. 142 and 144, Denham and Atherstone ... ..	New West { Open ... Coast ... { Covered ...	90 2.5	2247.0	{ 0.0007 calculated.	Gower-Bell Telephone. Speak- ing good.
London and Denham ...	Underground ... ..	21	2968.5	{ 0.0009 calculated.	Ditto
Nos. 142 and 144, Denham and Stafford ... ..	New West { Open ... Coast ... { Covered ...	119 5	4715.7	{ 0.0015 calculated.	Gower-Bell Telephone. Speak- ing fairly good.
South Wales ... ..	... .. { Open ... ... .. { Covered ...	72 1.35	5680.0	{ 0.0016 calculated.	Gower-Bell Telephones, work- ing satisfactorily.
Newcastle Telephone system, Warkworth to Stockton P.O. ... ..	{ Via Newcastle, Sunderland, and West Hartlepool... }	63.831 12.089	5817.6	{ 0.0016 calculated.	Gower-Bell Telephones. 5 in- termediate electro-magnets of 1000 ohms resistance, in derived circuit. Speaking satisfactory.
Nos. 142 and 144, London and Towcester ... ..	New West { Open ... Coast ... { Covered ...	50 21	7941.8	{ 0.00234 calculated.	Gower-Bell Telephone. Speak- ing weak, but just able to hold a conversation.
Nos. 142 and 144, Denham and Nantwich ... ..	Ditto { Open ... { Covered ...	145 7	7612.1	{ 0.0034 calculated.	Gower-Bell Telephone. Speak- ing weak, but just able to carry on a conversation.



## ORIGINAL COMMUNICATION.

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Wire.	Route.	Length in miles.	Total resistance $\times$ capacity.	Speed of current in seconds.	Telephonic notes.
London to Denham (with additional loop between Denham and Hanwell)	Underground ... ..	39	10221.0	0.0032 calculated.	Gower-Bell Telephone Speak- ing good, but approaching the point of difficulty.
Nos. 142 and 144, Denham to Warrington ... ..	New West { Open... Coast ... { Covered ...	172 8	10399.16	0.0038 calculated.	Only just able to speak. Impos- sible to carry on conversation.
Nos. 142 and 144, London to Atherstone ... ..	Ditto { Open ... { Covered ...	90 23.5	12511.2	0.004 calculated.	Only just possible to speak.
New Irish Cable ... ..	Cable ... { Open... { Covered ...	25 62.9	12567.4	0.00412 observed.	Berliner's Telephone. Only just able to talk.
Newcastle District ... ..	... .. { Open... { Covered ...	30 30	12640.0	0.00412 calculated.	Requires very good telephones and very good voices.
Copper. London to Nevin (land lines for the new Irish Cable) ... ..	{ Via Worcester } Open... and { Covered (Shrewsbury ...)	251.18 18.5	15771.5	*0.0035 observed.	Berliner's Telephone. Some voices good.
No. 11, London Stock Ex- change to Liverpool Stock Exchange (Iron wire, No. 8 gauge) ...	Via London { Open... and N.W. { Covered Railway ...}	193.014 7.248	16417.8	0.005 observed.	Telephone not tried.

N.B.—All the open wires are iron except those marked copper, the speed is calculated from the formula  $t = 32 \times 10^{-3} K R$ .\* The constant for copper is  $23 \times 10^{-3}$ .

If we put equation (1) into this form,

$$A = krx^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and give to A the following values :—

Copper (overhead) .....	15,000
Cables and underground ...	12,000
Iron (overhead) .....	10,000

we can find the limiting distance we can speak with any wire; for

$$x^2 = A/kr.$$

Take copper, whose constant is 15,000, and a wire whose resistance is 1\* per mile, and capacity 0·0124 per mile, then—

$$x^2 = \frac{15000}{0\cdot0124},$$

$$x = 1100 \text{ miles,}$$

which is the limit of speaking upon such a wire.

This law has been arrived at independently by M. Vachy, of Paris, and by Dr. Wietlisbach, of Berne, and it has been verified on the German underground wires by Messrs. Felten & Guilleaume.

The difference between copper and iron is clearly due to self-induction, or to the electro-magnetic inertia of the latter, and the difference between copper overground and copper underground is due to the facility that the leakage of insulators offers to the rapid discharge to earth at innumerable points, of the static charge, which in gutta-percha-covered wire can find an exit only at the ends.

W. H. PREECE.

# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM,

Appearing in the principal English and Foreign Technical Journals, etc.

(*Philosophical Magazine*, Vol. 22, No. 137, Oct. 1886.)

- O. HEAVISIDE**—Self-Induction of Wires. **T. C. MENDENHALL**—Electrical Resistance of Soft Carbon under Pressure. **T. GRAY**—New Standard Sine Galvanometer.

(No. 138, Nov., 1886.)

- T. GRAY**—Electrolysis of Silver and Copper, and Standardising of Current and Potential Meters thereby. **O. HEAVISIDE**—Self-Induction of Wires. **H. TOMLINSON**—Effect of Stress and Strain on the Electrical Resistance of Carbon.

(No. 139, Dec., 1886.)

- LORD RAYLEIGH**—Self-Induction and Resistance of Compound Conductors. **E. H. M. BOSANQUET**—Magnetic Decay of Permanent Magnets. The Tension of Lines of Force in Electro-Magnets.

(Vol. 23, No. 140, Jan., 1887.)

- O. HEAVISIDE**—Self-Induction of Wires. **T. GRAY**—Silk versus Wire Suspensions for Galvanometers.

(No. 141, Feb. 1887.)

- G. CAREY FOSTER**—Method of determining Coefficients of Mutual Induction. **E. H. M. BOSANQUET**—Silk v. Wire (as a Suspension for Galvanometers). **O. HEAVISIDE**—Self-Induction of Wires.

(No. 142, March, 1887.)

- LORD RAYLEIGH**—Behaviour of Iron and Steel under the Operation of Feeble Magnetic Forces. **W. BROWN**—Effects of Percussion in changing the Magnetic Moments of Steel Magnets.

(*Nature*, Vol. 34, No. 884, 7th Oct., 1886.)

- G. CAREY FOSTER**—Tangent Galvanometer.

(No. 886, 21st Oct., 1886.)

- J. RENNIE**—Tangent Scale in a Galvanometer.

(Vol. 35, No. 898, 9th Dec., 1886.)

- A. P. LAURIE**—Electric Charge on the Atoms.

(No. 895, 23rd Dec., 1886.)

**J. C. McCONNELL**—Error in Maxwell's Deduction of Equations of Ind Currents from Electro-dynamical Laws. **T. HIGGIN**—Elect Phenomenon.

(No. 896, 30th Dec., 1886.)

**H. D. GARDNER**—Electric Clock Alarm.

(No. 897, 6th Jan., 1887.)

**H. W. WATSON**—Error in Maxwell's Deduction (see ante).

(No. 900, 27th Jan., 1887.)

**H. W. WATSON**—Magnetic Theory.

#### PROCEEDINGS OF ROYAL SOCIETY.

(*Nature*, 9th Dec., 1886, p. 142.)

**J. BROWN**—Theory of Voltaic Action.

(*Nature*, 3rd Feb., 1887, p. 334.)

**Dr. J. HOPKINSON**—Note on Specific Inductive Capacity.

(*Nature*, 3rd Feb., 1887, p. 334.)

**Professor QUINCKE**—Dielectric Constants of Fluids.

(*Nature*, 24th March, 1887, p. 501.)

**W. H. PREECE**—Limiting Distance of Speech by Telephone.

(*Electrical Review of New York*, Vol 9, No. 5, 2nd Oct., 1886.)

The Bright Platinum-plating Process. Electric Smelting. The Roberts Battery.

(No. 6, 9th Oct., 1886.)

Eaton's Ammeters, Voltmeters, and Volt-am-meters.

(No. 8, 23rd Oct., 1886.)

New Multiple Series Distribution Box.

(No. 9, 30th Oct., 1886.)

The Plush Electro-magnetic Protector. Benjamin's Underground System Wires.

(No. 10, 6th Nov., 1886.)

**C. H. CROSS** and **W. E. SHEPARD**—The Inverse E.M.F. of Electric Arc.

(No. 12, 20th Nov., 1886.)

**L. LAURIOL**—Comparison between the different Systems of Transm Motive Power. Long-distance Telephony. **F. BIDLON**—Incandes Lighting from Arc-light Circuits.

(No. 16, 18th Dec., 1886.)

: Welding.

(*Electrician and Electrical Engineer*, Vol. 5, No. 58, Oct., 1886.)

- J. G. WHITE**—Heating of Aerial Conductors by Electric Currents (*continued*).  
*Anon.*—New System of Time Regulating by Electricity. **C. HERING**—  
 Dynamic Electricity (*continued*). **A. S. HICKLEY**—Carbons for Incan-  
 descent Lighting. **T. C. MARTIN**—Operation of Motors from Electric  
 Light Stations. **C. C. HASKINS**—Wire Joints.

(Vol. 5, No. 59, Nov., 1886.)

- C. HERING**—Dynamic Electricity (*continued*). **C. R. CROSS**—Melting  
 Platinum Standard of Light. **J. G. WHITE**—Heating of Aerial Con-  
 ductors by Electric Currents (*concluded*). **W. S. TURNER**—Proportion-  
 ing Conductors for a given Fall of Potential. **A. E. DOLBEAR**—  
 Electric Communication without Wires. **C. HERING**—Practical  
 Deductions from the Franklin Institute Tests of Dynamos. **C. C.**  
**HASKINS**—Electric Lighting and Aerial Wires. **H. LEMP**—Distribu-  
 tion of Arc and Incandescent Lamps on the same Circuit.

(Vol. 5, No. 60, Dec., 1886.)

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(*Journal Télégraphique*, Vol. 10, No. 10, Oct., 1886.)

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(Centralblatt für Elektrotechnik, Vol. 8, No. 26, 1886.)

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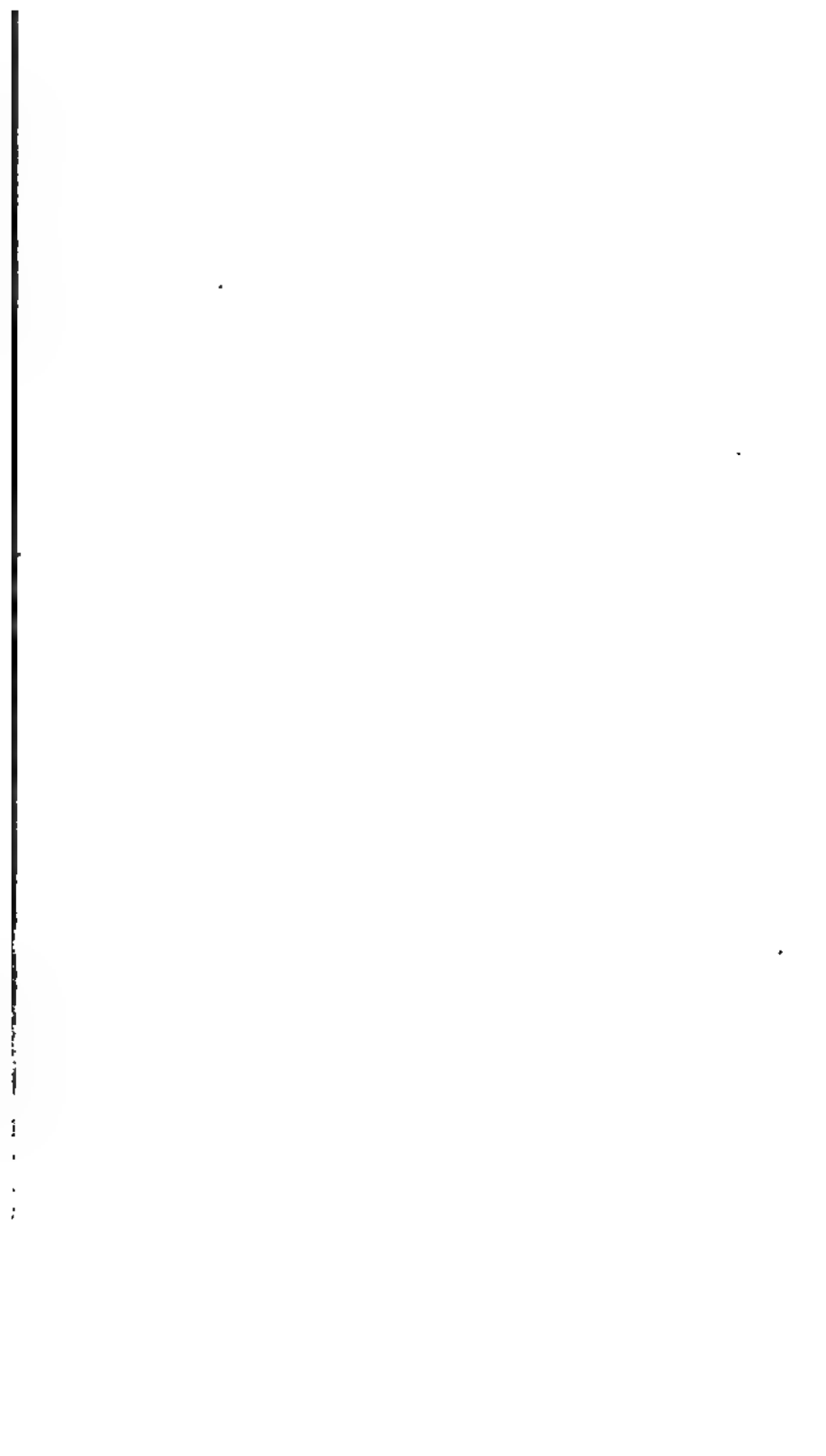
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# JOURNAL

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The One Hundred and Sixty-sixth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 28th, 1887—Sir CHARLES T. BRIGHT, M.I.C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

The following transfer was announced:—From the class of Associates to that of Members—Martin F. Roberts.

Donations to the Library were announced as having been received since the last meeting from Professor Zetzsche, Foreign Member; the Société Française de Physique; and Colonel R. Smith, R.E.; the latter's donation being a memoir of Colonel Sir J. U. Bateman-Champain, late President of the Society.

The SECRETARY stated that Mr. Ernest Danvers, Associate, had forwarded from Columbia a map showing the telegraph system in that country (where he was for some time Inspector-General of Telegraphs), but unfortunately it had been lost in transit.

The donations were acknowledged by a unanimous vote of thanks.

The following paper was then read by Professor Ayrton:—

## MODES OF MEASURING THE COEFFICIENTS OF SELF AND MUTUAL INDUCTION.

By Professor W. E. AYRTON, F.R.S., and JOHN PERRY, F.R.S.,  
Members.

This paper does not resemble Professor Hughes' Inaugural Address, in containing a charming account of new experimental researches into the laws of self-induction and of the extra resistance offered to a rapidly varying current: its aim is different, it is meant to help the practical electrician to obtain as clear a conception of self-induction as he now has of resistance, and to this end the paper contains the description, and theory of the action, of a simple apparatus that we devised a year ago, and which we have been since improving, for the *direct* measurement of the coefficients of self and of mutual induction in terms of the legal unit.

The electrician of to-day is so familiar with the idea of resistance, that he is liable to forget the time when people had no clear conception that the resistance of a conductor was one of its definite properties, like its weight and length. In the "Reports of the Committee on Electrical Standards," edited by the late Professor Fleeming Jenkin, a most interesting account is given of the gradual growth of the conception that a conductor had a definite resistance; and in our paper on "A New Determination of the Ratio of the Electromagnetic to the Electrostatic Unit of Electrical Quantity," read before this Society in 1879, something is said about the development of this idea of resistance. And during the fifty years that have elapsed since Lenz employed the 1 foot of No. 11 copper wire as his unit of resistance, the electrical world has not only learnt to regard resistance as a definite property of a definite piece of matter in a definite state, but has become so fully imbued with this idea, that it positively resented Professor Hughes' experimental proof, last year, that the resistance of a conductor for an intermittent current was a variable.

Now, how has this clear perception about resistance been acquired? We believe by the measuring of hundreds of thousands

of resistances during the last twenty-two years in terms of a unit of resistance with a simple name; and we feel that it will not be until many measurements of the coefficients of self-inductions of various coils, electro-magnets, dynamos, etc., have been made by practical men, as part of their regular work, and expressed in a unit of self-induction having a simple name, that they will come to have the same instinctive feeling about self-induction that they now have about resistance. There are two reasons why such measurements have not been regularly made: one is, that the importance of a knowledge of the coefficient of self-induction of a circuit is only beginning to be generally appreciated; the other, that the methods of measuring the coefficient have not been very easy to employ. But now that the speed of telegraph signalling is becoming greater and greater, that long-distance telephony is receiving more and more attention, and that the system of distributing electrical energy with secondary generators and alternating currents has become a most important rival to other systems, the regular measurement of the coefficients of self and of mutual induction of dynamos, electro-magnets, induction coils, etc., has now become quite as important to the electrical engineer as the measurement of resistance.

Professor Hughes' telephone methods for comparing the self-induction of one circuit with that of another are marvellously sensitive, as shown by the results he obtained, but the very delicacy of the methods prevents their being used without a compensator or adjustable standard of mutual induction for the commercial measurement of a coefficient of self-induction in terms of the product of a time into a resistance.

On turning to the electrician's "stand-by," Clerk Maxwell, one finds a complete exposition of the general principles of self-induction, also several methods for determining what he calls "the Electromagnetic Capacity of Self-Induction of a Coil." The very length of the name makes one fear that the methods will be impracticable for every-day work, and that is the case; for the only one which is suited to a practical man, as far as the apparatus is concerned, is unsuitable from a difficulty met with in carrying it out. The method in question is called "Comparison of the

"Electrostatic Capacity of a Condenser with the Electromagnetic 'Capacity of Self-Induction of a Coil,'"\* and the result expresses the coefficient of self-induction as equal to the product of two resistances into the capacity of a condenser. A number of attempts to employ this method were made rather more than a year ago by one of our students, Mr. Sumpner, but the necessity of having to make two separate adjustments of the resistances of the Wheatstone's bridge—one in order that there should be no deflection of the galvanometer for steady currents in the arms, the other that there should be no deflection on making or breaking the battery circuit—and the fact that altering the resistances to make either of these adjustments generally disturbed the other adjustment previously made, rendered the method nearly hopeless.

If not merely the resistance of the arms of the bridge were adjustable, but also the capacity of the condenser, so that it might be made to have different known values, this method might give good results if a galvanometer with a long periodic time of vibration were employed. If, however, an ordinary bridge galvanometer were used, the difference that exists in the rate of charging of a condenser and the rate of variation of the extrn current due to self-induction would make it impossible for the resistances and the capacity of the condenser to be generally adjusted, so that there was no deflection of the galvanometer on making and breaking the battery circuit, as there would first be a deflection on one side of the zero, then on the other, consequently a very accurate measurement of the coefficient of self-induction could not be made. Modifications of this method, by combining resistances and one or more condensers with a differential galvanometer, have been suggested and tried with more or less good results, but, as explained later on, with none of these

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\* A student might naturally conclude that, as a capacity measured *electrostatically* is of the same dimensions as a coefficient of self-induction measured *electromagnetically*, both being a simple length, it was this comparison that Clerk Maxwell referred to; but that is not the case, as shown by the working out of the problem, consequently the answer contains the square of a resistance in addition to the capacity of the condenser and the coefficient of self-induction of the coil, so that the problem is not strictly the comparison of the electrostatic capacity of a condenser with the electromagnetic capacity of self-induction of a coil.



methods can the effect be made *cumulative*, and the test therefore very sensitive, by using the plan of rapidly making and breaking the battery circuit.

Probably the best method yet published for measuring the coefficient of self-induction in terms of the product of a time into a resistance is that described by Lord Rayleigh in his paper on "Experiments to Determine the Value of the British Association Unit of Resistance in Absolute Measure," given in the "Philosophical Transactions" for 1882, and abstracted by one of us for the Journal of the Society, where it will be found on page 152 of the volume for that year. This method, based on one devised by Clerk Maxwell, but not described in either the first or the second edition of his book, consists in placing the coil with self-induction in one of the arms of a Wheatstone's bridge, and first obtaining balance in the ordinary way, the battery circuit being completed *before* the galvanometer circuit, then measuring the swing of the galvanometer needle when the battery circuit is completed *after* the galvanometer circuit, and lastly measuring the steady deflection obtained when one or more of the arms of the bridge is altered by a known amount, the battery circuit being completed before the galvanometer circuit as in the first test.

This method is simple, but it has three great disadvantages. First, while an ordinary Wheatstone's bridge is employed, an ordinary dead-beat Wheatstone's bridge galvanometer is totally unsuitable, since on making or breaking the battery circuit, when performing the second test, the galvanometer needle must not begin to move until the extra current has ceased; and further, the time of vibration of the galvanometer needle, and the logarithmic decrement, have to be determined, as they both enter into the formula, so that in fact a ballistic galvanometer must be employed. Second, if the coefficient of self-induction to be measured be small, the swing of the galvanometer needle obtained on making or breaking the battery circuit will be very small, and cannot be accurately read. And the effects cannot be made *cumulative* by rapidly making and breaking the battery circuit when using either this method or the one described in

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These *Maxima*... referred to above, or any simple modification of them *cannot*, since the swings of the galvanometer needle are in opposite directions on making and on breaking the battery circuit. Third, in making the first test with the Wheatstone's bridge a very accurate balance must be obtained, since the swing *arises*, on making the second test may be entirely due to the effect of self-induction. Consequently a sliding wire in addition to the ordinary bridge must be employed, instead of using the simple interpolation method of obtaining the last decimal place from the small steady deflections to the right and left of the true zero. (*See Appendix II.*)

A circuit containing self-induction acts as if it had a larger resistance than its true one when a current is started in it, and a smaller resistance when the current is stopped. Hence, if balance be obtained with a Wheatstone's bridge in the ordinary way, the fact of any of the arms possessing self-induction, or of any one of the arms having a condenser attached to it, will produce no effect on the balance if the battery circuit be rapidly made and broken, provided that the rapidity of make and break be not too great for the currents in the arms of the bridge to reach their steady values each time that the battery circuit is made, and to die away each time that it is broken. If the currents have not time to reach their steady value when the battery circuit is closed, and to die away when it is broken, then self-induction in any one of the arms will produce a disturbance in the balance; but such a method of measuring a coefficient of self-induction would lead to very complicated formulæ, and is not worth developing with the view of obtaining a simple method of measuring self-induction.

It therefore occurred to us to consider whether, without employing such rapid makes and breaks as would prevent the currents reaching their steady values, the self-induction of a circuit might not be made to act as an apparent steady definite increase of the resistance of that circuit which could be measured in the ordinary way with a Wheatstone's bridge or differential galvanometer, and by this means the measurement of a coefficient of self-induction would simply resolve itself into the

measurement of a resistance. And this problem we solved in the following way, in the spring of 1886:—

The coil, the coefficient of self-induction of which it is desired to measure, is placed in one of the arms of a Wheatstone's bridge, the three other arms consisting of ordinary doubly-wound resistance coils possessing no appreciable self-induction, and not only is the battery circuit rapidly made and broken, but, in addition, after each closing of the battery circuit the galvanometer circuit of the bridge is either short-circuited or broken, so as to cut out the galvanometer, and after each breaking of the battery circuit the galvanometer circuit is either unshort-circuited or closed again, so that the galvanometer is now operative again. In this way all the successive impulses of the galvanometer needle that are produced on starting the current in the coil with self-induction produce their *cumulative* effects, but the successive impulses of the needle that, under ordinary circumstances, would be produced on the needle in the opposite direction are cut out. Hence the self-induction possessed by one of the arms causes that arm to apparently increase in resistance by a definite amount depending on the coefficient of self-induction and the number of operations performed per minute. This apparent increase of resistance produces a deflection of the galvanometer which can be noted, and its value ascertained by comparing it with the deflection produced with steady currents when one or more of the arms of the bridge is altered by a known amount, as in making the Rayleigh test (page 295). But since the necessity of having to read the deflection limits the speed of performing the double make and break operation, in order that the spot of light may not be sent off the scale, we soon replaced this comparative deflection *cumulative* method\* by a much more sensitive zero

\* Since this paper was announced to be read, M. Lippmann has communicated to the French Academy a paper "Sur la détermination du coefficient du Self-Induction," by MM. P. Ledeboer and G. Maneuvrier, describing a new method which these experimenters have devised. An examination of it, however, shows that it is identical with our *less sensitive* 1886 method. After giving some very accurate results that they have obtained by using the new method of testing, they say: "Nous l'avons préférée d'ailleurs aux autres méthodes pour la mesure des faibles coefficients de self-induction, et



*delicate one for the absolute measurement of a coefficient of self-induction, and the coefficient is expressed in its simplest possible form, namely, as the product of a time into a resistance.*

If, instead of periodically cutting out the galvanometer circuit while the battery circuit is being broken, we cut it out while the battery current is being made, and allow the galvanometer to be operative while the battery circuit is being broken, the resistance of the coil possessing self-induction will apparently become less by a definite amount, and in this case  $\sigma$  will be the apparent diminution of its resistance, and  $T$  is the time the galvanometer is operative after the battery circuit is broken.

The first apparatus for enabling measurements of self-induction to be made in this way was constructed in the spring of 1886, under the superintendence of one of our assistants, Mr. Mather. It consisted of a double commutator, shown in Fig. 1, the spindle,

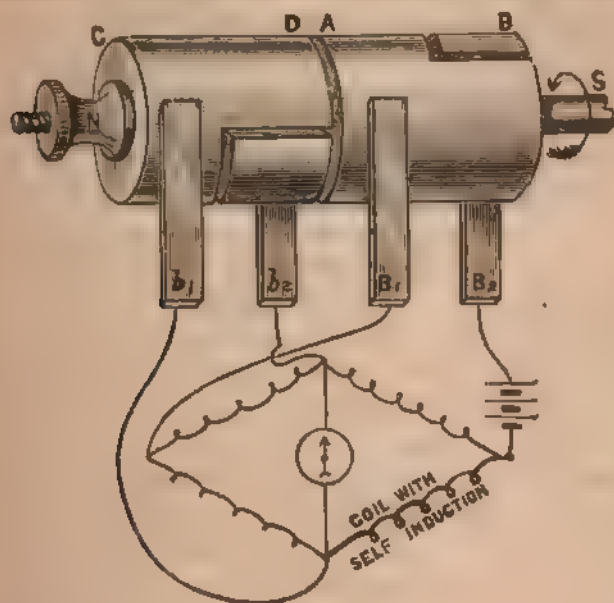


FIG. 1.

$S$ , to which the commutators were locked by the nut,  $N$ , being rotated at any speed by a small electro-motor not shown in the figure, to which was attached a Young's speed-indicator, which registered the speed of rotation at any moment. The brushes,



$B_1, B_2, b_1, b_2$ , were fixed to the baseboard and joined to the bridge, as indicated in the figure. When the double commutator was rotated by the motor (of which the speed was correctly adjusted by means of a Varley's flexible carbon-resistance), the portion  $A B$  caused the battery circuits to be periodically made and broken, while the other portion,  $C D$ , periodically short-circuited and unshort-circuited the galvanometer, so that the following cycle of operation, called for simplicity *one operation*, was performed any desired number of times per minute.

Battery Circuit.	Galvanometer Short Circuit.
Make.	While broken.
While made.	Make.
Break.	While made.
While broken.	Break.
Make.	While broken.

The time,  $T$ , in the formula given above is the interval that elapses in one rotation between the time when the continuous part of the commutator  $A B$  comes into contact with the brush  $B_2$ , and the time when the continuous part of the commutator  $C D$  comes into contact with the brush  $b_2$ . This time can be made shorter or longer by varying the speed of rotation, but it can also be varied by loosening the nut,  $N$ , and shifting one of the commutators round relatively to the other, and then screwing up the nut again. Indeed, it was for the purpose of enabling this adjustment to be easily made that double springs were in each case employed, instead of making one contact through the spindle.

At about this time, that is, in the summer of last year, we invited Mr. Sumpner, one of the third year students of the Central Institution, to experiment with this apparatus, and generally, by experimenting and by working at the subject mathematically, to find out the capabilities of this new method of measuring self-induction, also to see whether by possibly using

the apparatus in some other way the method could be made even still more sensitive. We take this opportunity of thanking Mr. Sumpner for the energetic way in which he has worked at the subject, and for the very large number of experiments on the accuracy of various modes of measuring self-induction which he has conducted, with the occasional aid of two other of the third year students, Messrs. Rossiter and Watney.

The various methods of testing which Mr. Sumpner has employed, the mathematical development of the formulæ used by him in each case, and his general conclusions as to the accuracy of the various methods, are given later on in the Appendices; but one interesting result that he has arrived at may here be mentioned, viz., that although the apparatus has during the past twelve months been gradually totally altered in detail, the experiments have shown that the principle originally proposed for carrying out the measurement, requiring the cycle of operations given in the foregoing table to be periodically and rapidly performed, is the simplest and most accurate one to employ. One reason for the accuracy of the method is that neither commutator is in any one of the arms of the bridge, so that no ordinary change in the resistance of the contacts produced by oil or dust, etc., getting in between the springs and the rotatory barrels can introduce any error into the test. Of course an actual failure of contact might lead a careless experimenter to a wrong result, but so might a disconnection in the battery or galvanometer circuit in an ordinary Wheatstone's bridge, if the conclusion was drawn that, because there was no deflection on pressing down the key, balance had been obtained.

The first thing to be done after constructing the apparatus, was to ascertain what was the formula connecting the unknown coefficient of self-induction with the apparent increase of resistance of the coil, and the time in which the cycle of operations was performed.

This formula we calculated ourselves, neglecting the self-induction of the galvanometer, since we felt that its effect could not be serious. Mr. Sumpner, on the other hand, took up the complete problem, and we give his proof reduced to its simplest

form, since, in addition to its application to the experiment in question, the calculation is interesting in connection with the problem of measuring the capacity of a condenser with a Wheatstone's bridge and vibrating wippe, which will be found worked out by Clerk Maxwell, without, however, any notice being there taken of the self-induction of the galvanometer, the effect of which, therefore, on the answer is not considered.

Fig. 2 shows the Wheatstone's bridge,  $s$  being the resistance

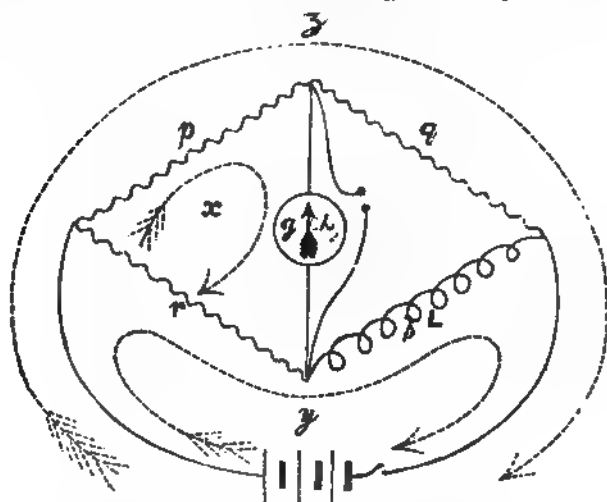


FIG. 2.

of a coil with a coefficient of self-induction  $L$ ;  $p$ ,  $q$ , and  $r$  are the resistances of the three other arms of the bridge when they are so adjusted that there is no deflection of the galvanometer on rotating the commutators at a certain speed. On rotating the commutators, a current will sometimes pass one way through the galvanometer, and sometimes the other, but with the particular speed of rotation  $p$ ,  $q$ , and  $r$  are such that the total quantity passing through the galvanometer is nought. The resistance and coefficient of self-induction of the galvanometer are  $g$  and  $\lambda$ , and the electro-motive force and resistance of the battery  $E$  and  $b$ . Then, if  $x$ ,  $y$ ,  $z$  be the imaginary currents on the meshes,\*

\* For the method of solving such problems by the employment of these imaginary currents, see a most interesting paper by Dr. Fleming, in the *Proceedings of the Physical Society*, vol. vii., p. 215.

$$r(x - y) + p(x + z) + gx + \lambda \frac{dx}{dt} = 0$$

$$b(y + z) + r(y - x) + sy + L \frac{dy}{dt} = E$$

$$b(y + z) + px + qz = E$$

Eliminating  $y$  and  $z$ , we have

$$A x + B \frac{dx}{dt} + P \frac{d^2 x}{dt^2} + C = 0 \quad \dots (2)$$

$$\text{where } A = \begin{vmatrix} r + p + g & -r & p \\ -r & s + b + r & b \\ p & b & p + q + b \end{vmatrix}$$

$$B = \lambda \begin{vmatrix} s + b + r & b \\ b & p + q + b \end{vmatrix} + L \begin{vmatrix} p + g + r & p \\ p & p + q + b \end{vmatrix}$$

$$P = (p + q + b) L \lambda,$$

$$C = E(p s - q r).$$

Integrating (2) we obtain

$$A \int x dt + B \int dx + P \int \frac{d^2 x}{dt^2} dt + \int C dt = 0 \quad \dots (3)$$

The effect on the galvanometer needle may be divided into two parts—one, the first rush of electricity that occurs through it when the battery circuit is closed during the time the galvanometer is not short-circuited, and the diminution of this current through zero and its increase in the opposite direction until it reaches a certain definite value, when the currents in the arms of the bridge have become steady; the second, the dying away of this reverse current in the galvanometer while it is short-circuited. These two effects we shall consider separately.

To ascertain the amount of the first effect, we must integrate between  $t$  equals nought and  $t$  equals  $T$ , since, by hypothesis,  $T$  is the interval that elapses between the closing of the battery circuit and the short-circuiting of the galvanometer.

Integrating between these limits, we have

$$\int_0^T x dt = Q_1,$$

where  $Q_1$  is the quantity of electricity that flows through the galvanometer in the time  $T$ ,

$$\int_0^T dx = -\frac{C}{A},$$

since this is the final steady value of the current  $x$  which is nought at time nought.

Also, 
$$\int \frac{d^2 x}{dt^2} dt = \frac{dx}{dt}$$

and 
$$\frac{dx}{dt} = 0 \text{ at time } T,$$

since, by hypothesis,  $T$  is long enough for the currents to become steady. To find the value of  $\frac{dx}{dt}$  at time nought, we have the fundamental differential equation for the current through the galvanometer

$$g x + \lambda \frac{dx}{dt} = V,$$

where  $V$  is the potential difference at the terminals of the galvanometer at any time. And since where  $t$  equals nought  $x$  equals nought, we have at time nought

$$\frac{dx}{dt} = \frac{V_0}{\lambda},$$

where  $V_0$  is the value of the potential differential at the terminals of the galvanometer at time nought

and 
$$V_0 = -\frac{E p}{p + q + b},$$

since at time nought both  $x$  and  $y$  are nought, therefore at time nought

$$\frac{dx}{dt} = -\frac{E p}{\lambda (p + q + b)},$$

and therefore 
$$\int_0^T \frac{dx}{dt} dt = -\frac{E p}{\lambda (p + q + b)} T.$$

Lastly, 
$$\int_0^T dt = T,$$

therefore equation (3) reduces to

$$A Q_1 - B \frac{C}{A} + P \frac{E p}{\lambda (p + q + b)} + C T = 0.$$

Next let us consider the dying away of the current in the

galvanometer after it is short-circuited. We may now use simply the equation

$$g x + \lambda \frac{d x}{d t} = 0,$$

if the resistance of the short circuit be very small compared with that of the galvanometer itself.

Multiplying by  $d t$ , and integrating from  $t$  equal  $T$  to  $t$  equal  $T'$ , say, the time the short circuit is removed, we have

$$g Q_2 + \lambda \int \frac{d x}{d t} = 0,$$

where  $Q_2$  is the quantity of electricity that passes through the galvanometer while short-circuited. At the moment after short-circuiting

$$x = - \frac{C}{A},$$

the steady value of the current, since the self-induction of the galvanometer prevents any instantaneous change in the current passing through it, and when  $t$  equals  $T'$  the current in the galvanometer is nought, since, by hypothesis, the time of short-circuiting  $T' - T$  is long enough for the current in the galvanometer to die away,

$$\therefore \int_T^{T'} \frac{d x}{d t} d t = - \left( - \frac{C}{A} \right).$$

Now in order that there may be no deflection of the galvanometer, we must have

$$Q_1 + Q_2 = 0,$$

$$\therefore \frac{B C}{A_s} - \frac{P}{A} \frac{E p}{\lambda (p + q + b)} - \frac{C}{A} T = \frac{\lambda}{g} \cdot \frac{C}{A}.$$

Let  $\sigma$  be the apparent steady increase of the resistance of the coil with self-induction (of which the ordinary resistance is  $s$ ) produced by the rotation of the commutator at speed corresponding with the times  $T$  and  $T'$ , then

$$p (s + \sigma) = r q,$$

$$\therefore C = - E p \sigma.$$

Substituting this value for  $C$ , and for  $P$  its value  $L \lambda (p + q + b)$  in the preceding equation, we have

$$L = \sigma \left( T + \frac{\lambda}{g} - \frac{B}{A} \right) \dots \dots \dots (4)$$

Now  $\frac{\lambda}{g}$  and  $\frac{B}{A}$  are usually small compared with  $T$ , as is shown in the more general proof, page 328, therefore when this is the case

$$L = \sigma T,$$

the simple formula previously referred to.

Referring to Fig. 1, it will be seen if the brushes  $B_1$  and  $b_2$  touch over a small area, and if the centres of these areas are in a plane passing through the axis of rotation, that  $T$  is simply equal to

$$\frac{\text{angle between the slits in the two commutators}}{360 \times \text{number of revolutions per second.}}$$

We may call

$$\frac{\text{angle between the slits in the two commutators}}{360^\circ}$$

the lead or  $l$ , and  $l$  will be equal to  $\frac{1}{4}$ , for example, when each of the cycle of operations given in the table lasts for one quarter of a revolution.

Introducing  $l$  we have, if  $\sigma$  be in ohms,

$$L = \frac{l}{n} \sigma \text{ second-ohms,}$$

where  $n$  is the number of revolutions of the commutator per second, or

$$L = \frac{60 l \sigma}{N} \text{ second-ohms} \quad \dots \dots (5)$$

where  $N$  is the number of revolutions per minute.

A large number of experiments were made in May, 1886, with a solenoid of which the resistance was about 4 ohms at ordinary temperatures, to see how nearly the coefficient of self-induction determined by using the double commutator arrangement running at different speeds, and with different leads, agreed with the coefficient of self-induction determined by Lord Rayleigh's method, which gave 0.0215 second-ohm for the solenoid alone, and 0.517 second-ohm when a certain iron core was introduced into the solenoid.

The following are some of the results obtained with the solenoid without the iron core when the lead

$$l = \frac{1}{4} \text{ about :—}$$



$N$	$s + \sigma$	$\sigma$	$L = \frac{60}{N} \sigma$
0	4.06		
660	6.70	2.64	0.0200
760	7.00	2.94	
860	7.50	3.44	0.0200
997	8.00	3.94	
1117	8.50	4.44	
1240	9.00	4.94	
1362	9.50	5.44	0.0200
1450	11.00	6.94	
1850	12.00	7.94	0.0204
2395	13.50	9.44	0.0202

Plotting these results on squared paper, we obtain the curve *AAA* (Fig. 3), the abscissæ representing the values of  $N$ , the number

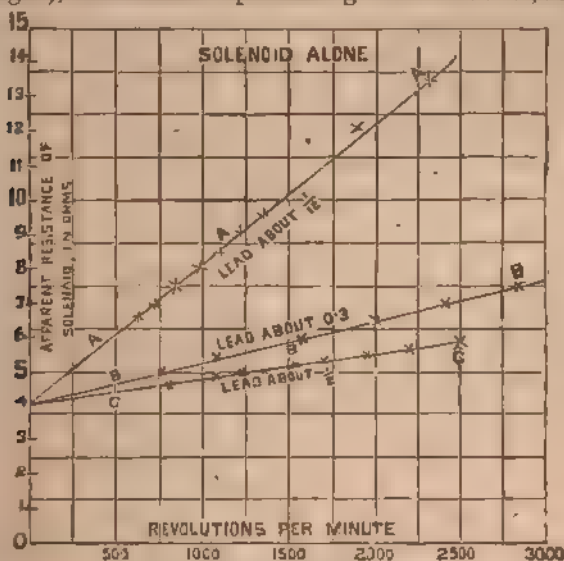


FIG. 3. IMPROVED BECHMETER.

of revolutions per minute, and the ordinates the values of  $s + \sigma$ , the apparent resistances of the solenoid at the various speeds. This curve we see is a perfectly straight line, which means that the increase of resistance is proportional to the speed; or, in other words, the approximate formula (5) for calculating  $L$  may be

employed with any of the results, and the value of  $L$  so obtained from a few of the results is given in the last column of the preceding table. That we may use the approximate formula at all for  $L$  shows us that the term

$$\frac{\lambda}{g} - \frac{B}{A}$$

in the expression (4) is negligible in comparison with  $T$ ; and that we may use it even for the result obtained with the highest speed of rotation employed, viz., 2,335 revolutions per minute, shows us

that  $\frac{1}{12 \times 2335}$  minute, or about 0.0022 of a second, which is the time the galvanometer remains unshort-circuited after the battery circuit is closed, is long enough for the currents in the bridge to reach their steady value.

The values of the coefficient of self-induction obtained, 0.020, are approximately equal to 0.0215 second-ohm, the value obtained by Lord Rayleigh's method, and that they are not exactly the same arises from the difficulty, already referred to, of estimating the lead  $l$  accurately with the eye. This would not introduce any inaccuracy in the actual use of the instruments, since for a given adjustment of the relative positions of the commutators  $AB$  and  $CD$  in Fig. 1, the value of  $l$  is a constant, and this value would be determined once for all, not by the eye, but by an actual experiment made with a coil of known self-induction. We were not, however, calibrating the instrument on the assumption that the approximate formula (5) was true, but using the instrument to see how nearly (5) might be employed. In our experiment  $l$  was probably not  $\frac{1}{11}$  exactly, but more nearly  $\frac{1}{11}$ .

Experiments were now made with the lead  $l$  equal about to 0.3.

$l = 0.3$  about.

$N$	$s + \sigma$	$\sigma$	$L = \frac{60 l \sigma}{N}$
0	4.00		Mean value, 0.0218 ohm.second.
770	5.00	0.94	
1120	5.50	1.44	
1600	6.00	1.94	
2000	6.50	2.44	
2410	7.00	2.94	
2860	7.50	3.44	

These results when plotted give the straight line *BBB*. Instead, therefore, of calculating the value of *L* from several of the results, as before, we may more accurately determine it from the point at the end of this line corresponding with a speed of 3,000 revolutions per minute, and an apparent resistance of 7.7 ohms, that is, an apparent increase of 3.64 ohms. This gives for *L* the value 0.0218 second-ohm, also not differing much from the true value 0.0215 second-ohm, considering that the exact value of the lead was not known.

Lastly, experiments were made with a lead of about  $\frac{1}{2}$ , with the following results:—

$$l = \frac{1}{2} \text{ about.}$$

<i>N</i>	<i>s</i> + <i>σ</i>	<i>σ</i>	$L = \frac{60 l \sigma}{N}$
0	4.1		
820	4.7	0.6	0.0219
1100	4.9	0.8	
1240	5.0	1.0	0.0217
1580	5.2	1.1	
1700	5.3	1.2	0.0212
1950	5.5	1.4	
2220	5.7	1.6	0.0216
2500	5.9	1.8	0.0216

Here again the points all lie very nearly in the straight line *CCC*, and the values given in the last column, calculated from some of the points, are all nearly equal to one another and to the true coefficient of self-induction of the solenoid.

It is clear, therefore, that with speeds up to 3,000 revolutions per minute and higher, and with leads varying from  $\frac{1}{4}$  to  $\frac{1}{2}$ , any single experiment correctly made gives the true value of the self-induction by the use of the simple formula

$$L = \frac{60 l}{N} \sigma.$$

We see from the formula, and from the experiments which substantiate it, that the smaller the lead and the higher the speed of rotation of the commutating arrangement, the larger is

the value of apparent increase of resistance, that is, the more sensitive is the test.

The iron core was now inserted in the solenoid, and a series of experiments made with leads equal to  $\frac{1}{16}$ ,  $\frac{1}{8}$ ,  $\frac{1}{4}$ ,  $\frac{1}{2}$ , in the respective sets of experiments. The results are shown by the curves *DDD*, *EEE*, *FFF*, *GGG* (Fig. 4). It will be observed

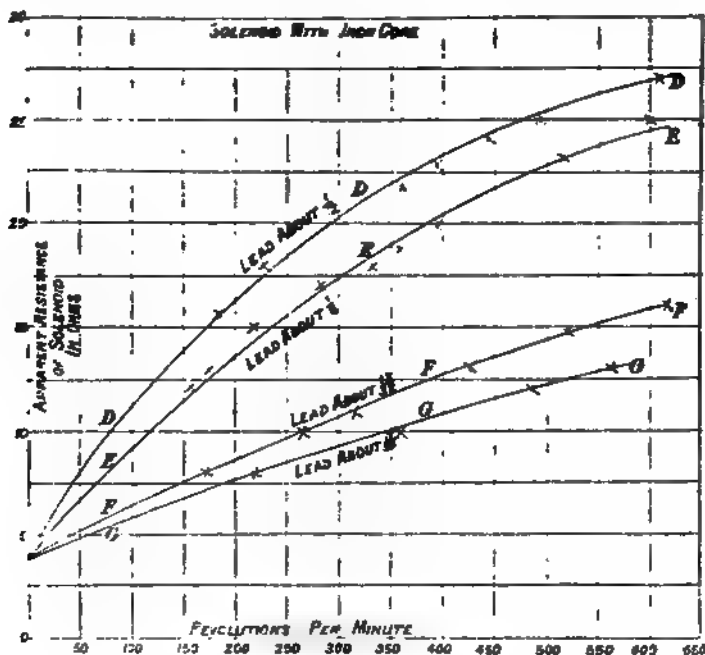


FIG. 4.

that, although the speed of rotation of the commutating arrangement did not exceed 680 revolutions per minute, the observations do not lie even approximately in a straight line, until the lead  $\frac{1}{16}$  is employed, and even when the last lead,  $\frac{1}{16}$ , is used there is still curvature in the line. This great difference between the curvature of the lines for a range of even slow speeds when the iron core is introduced, and the perfect straightness of the lines beyond the highest speed, 2,800 revolutions per minute, employed with the solenoid alone, arises from the coefficient of self-induction of the solenoid, with the iron core inserted, being about twenty-five times as great as the self-induction of the solenoid alone—

Hence the smallest interval,  $T$ , between the closing of the battery circuit and the short-circuiting of the galvanometer that can be employed is much larger when the iron core is inserted than when the solenoid alone is used. Consequently the lead must be large or the speed must be slow for the results to lie in a straight line. The curve  $DDD$ , obtained with the lead of about  $\frac{1}{12}$ , is too far from straight for any portion of it to be used in calculating the coefficient of self-induction, with the simple formula; in other words, so small a lead was unsuitable to be used. The greater part of the next curve,  $EEE$ , is also too far from straight to be used; but the first observation, corresponding with a speed of 156 revolutions per minute and an apparently increasing resistance of 8 ohms, is on a portion of the curve sufficiently straight to give an answer not much smaller than the truth. The result so obtained is

$$L = \frac{60 \times 1}{156 \times 6} \times 8 \\ = 0.513 \text{ second-ohm,}$$

which is not very much less than the true value of the coefficient of self-induction of the solenoid with the iron core, as determined by Lord Rayleigh's method, which, as before mentioned, gave 0.517 second-ohm.

With the lead of about  $\frac{1}{8}$  the lowest speed at which an observation was taken was 170, corresponding with an apparent increase of resistance of 4 ohms. This gives

$$L = \frac{60 \times 13}{170 \times 36} \times 4 \\ = 0.510.$$

If we take the next speed, 260, corresponding with an increase of resistance of 6 ohms, we find  $L$  equal to 0.500.

The last curve,  $GGG$ , obtained with the largest of the leads, about  $\frac{1}{4}$ , is the one that, being straightest, will probably give the best results, but as even this is not quite straight we must expect to obtain slightly too low a value. The two first observations correspond with speeds of 220 and 355 revolutions per minute, producing an apparent increase of resistance of 4 and 6 ohms. The values of  $L$  obtained from these results are 0.515 and 0.482

second-ohm, from which we should conclude that the true answer was a little higher than 0.515 second-ohm, and this we know is correct, as the true answer is 0.517 second-ohm.

That it is necessary to employ only slow speeds when the coefficient of self-induction of a coil is large compared with its resistance is no disadvantage, since, when this is the case, the apparent percentage increase of resistance at even slow speeds is great, and so can be accurately measured.

The preceding series of experiments have been made simply for the purpose of experimentally investigating the law of the instrument, and it is of course not at all necessary to make all these observations for the purpose of measuring the coefficient of self-induction in any particular case, nor is it necessary that the commutator should be able to be run at a number of known speeds. If the instrument is to be used to measure coefficients of self-induction the values of which do not vary between very wide limits, then it is sufficient if the instrument be constructed so as to run at one fixed speed which is not too high for the currents to become steady with the largest coefficient of self-induction that the instrument is designed to measure. If the instrument be required to measure coefficients of self-induction all having rather a large value, and also coefficients of self-induction all having rather a small value, then it would be desirable to construct the instrument so that it would run at either one low known speed or at one high known speed. The greatest sensibility in the measurement of coefficients of self-induction of which the values range within wide limits is obtained by constructing the instrument so that it can run at a number of known speeds. In practice, such a wide range instrument would be used as follows:—Before the instrument left the maker's hand the lead would be fixed and experimentally accurately determined once for all, and  $60 \times l$  might be called the constant of the instrument  $K$ . An experimenter, in determining the coefficient of self-induction of any coil, would—

*First.* Measure the resistance of the coil in the ordinary way.

*Second.* Rotate the instrument at some convenient speed, which must be noted, say  $N$  revolutions per minute, and by alter-

ing the resistance in one of the arms of the bridge until the galvanometer needle again comes to zero, observe the apparent increase of resistance of the coil,  $\sigma$  say.

*Third.* Rotate the instrument at some other convenient measured speed, say  $N'$ , and measure the apparent increase of resistance of the coil, say  $\sigma'$ , above its original value.

Then if 
$$\frac{\sigma}{N} = \frac{\sigma'}{N'},$$

the apparent increase of resistance obtained with either speed may be used, and the required coefficient of self-induction

$$= K \frac{\sigma}{N};$$

if, on the other hand,  $\frac{\sigma}{N}$  is larger than  $\frac{\sigma'}{N'}$ , then the speed  $N'$  is too high, and possibly also is  $N$ , to give accurate results. Therefore use a speed  $N''$  lower than  $N$ , and measure the new increase of resistance  $\sigma''$ .

If now 
$$\frac{\sigma}{N} = \frac{\sigma''}{N''},$$

the speed  $N$  was not too high to give accurate results, although  $N'$  was; hence the apparent increase of resistance obtained with either the speed  $N$  or  $N''$  may be used, and the required coefficient of self-induction

$$= K \frac{\sigma}{N}.$$

If  $\frac{\sigma}{N}$  be only a little greater than  $\frac{\sigma'}{N'}$ , and if  $N'$  much exceeds  $N$ , then it will not be necessary to make a third experiment at the lower speed  $N''$  unless very accurate results are desired, for the coefficient of self-induction can only be a very little greater than

$$K \frac{\sigma}{N},$$

which may therefore be practically taken as the correct value.

*By the simple addition, therefore, of such a commutating arrangement as we have described to an ordinary Wheatstone's bridge, it becomes possible, whenever the resistance of a coil, electro-magnet, etc., is being measured, to measure also the coefficient of self-induction in absolute measure, by a zero method*



which is as sensitive for the measurement of self-induction as the ordinary Wheatstone's bridge method is for the measurement of resistance.

From what precedes it follows that

$$L = K \frac{\sigma - \sigma''}{N - N''};$$

so that if  $s_1, s_2$  be the apparent resistances of the coil at two speeds,  $N_1, N_2$  revolutions per minute, the speeds not being too large,

$$L = K \frac{s_1 - s_2}{N_1 - N_2};$$

hence the coefficient of self-induction can be determined from two experiments made with the revolving double commutator without knowing the true resistance of the coil at all.

If there be doubt as to whether the speeds  $N_1$  and  $N_2$  are too large, a third check speed,  $N_3$ , may be used; or instead, as explained in Appendix II., a rough test of the resistance of the coil for steady currents may be made; but time need not be spent in obtaining an accurate balance, as quite a rough balance can be used as follows:—Let  $O$  on the scale be the true zero for no current passing through the galvanometer, and let the spot of light be at, say, 156 divisions when an approximation is made to balance for steady currents, the resistance of the coil appearing to be  $s$  ohms. Then the zero to be used in making the *second* and *third* tests just described is  $l \times 156$ , where  $l$  is the lead and  $\sigma$  is to be reckoned as the increase above the approximate value of the resistance for steady currents.

### COMMERCIAL FORMS OF THE INSTRUMENT.

The long-range instrument previously described requires an electro-motor to drive it, and a speed-indicator to register its speed, hence it would be too cumbersome for everyday work. Further, as it is very difficult to maintain the absolute constancy of the speed of a small motor driving a friction brake, like our commutating arrangement, at a high speed for any considerable time, in consequence of very slight variations in the friction of a high-speed brake producing marked variations in the speed, it requires two experimenters to use the form of our instruments already

described; one to vary the resistance until the galvanometer needle comes to zero, and the other to note the speed of the motor at the moment that the first experimenter gives a signal that the galvanometer needle is at zero.

Hence, in the autumn of last year, we were led to consider what form should be given to the commercial instrument to be used for the measurement of self-induction in this way. The main condition of the problem was that the instrument should be able to be used by one experimenter *alone*, since one experimenter alone can measure a resistance with a Wheatstone's bridge, and therefore we felt that if another experimenter had to be summoned when a coefficient of self-induction had to be measured, the instrument would lose much of its practical value. Our first idea was to employ clockwork that would drive the commutating arrangement at various fixed speeds, determined by the adjustment of a certain powerful governing arrangement, and to this end commutators of very small diameter were constructed, so as to waste only a small amount of power in friction and interfere but little with the action of the governor. The problem at first sight appeared an easy one, but the experiments of one of our assistants, Mr. Bourne, whom we have to thank for the many ingenious devices that he has suggested, and for the skill that he has brought to bear in the construction of the various experimental forms that the apparatus has passed through in its development, showed that in consequence of the high speed of the commutator, and of its acting as a friction brake, good results could only be obtained with clockwork of such unwieldy dimensions as to make it impossible for the instrument to be made portable; further, it was contrary to our engineering instincts to employ an apparatus which wasted so large a portion of the power required to drive it in the governing arrangements.

When making the experiments with the original commutating apparatus, two methods suggested themselves of obtaining a balance—the one, to try and to keep the speed of the electro-motor nearly constant, and to vary the resistance of one of the arms of the bridge, by trial, until the galvanometer needle came to zero; the other, to commence by increasing one of the arms of

the bridge by a definite amount, and to vary the speed of the electro-motor, by trial, until the galvanometer needle came to zero; and it soon appeared that the second method was the one that would have to be adopted in a practical instrument, since, while it is very difficult to maintain a constant speed, there is no difficulty in obtaining a gradually diminishing or increasing speed. For example, the apparatus may be provided with a fly-wheel having a large moment of inertia, and may be driven by hand more or less fast until the galvanometer needle comes to zero. The next step in order to avoid the necessity of a second experimenter was to devise a *recording speed-indicator*, and the best form that we have arrived at is based on the principle that we employed in constructing an apparatus some years ago for experiments on centrifugal force, described at a meeting of the Physical Society at the time, and in regular use in the mechanical laboratory of the Finsbury Technical College ever since, for experiments on centrifugal force.

Attached to the commutator of our self-induction apparatus is a box, *B* (Figs. 5), fitted with weighted elastic sides made of

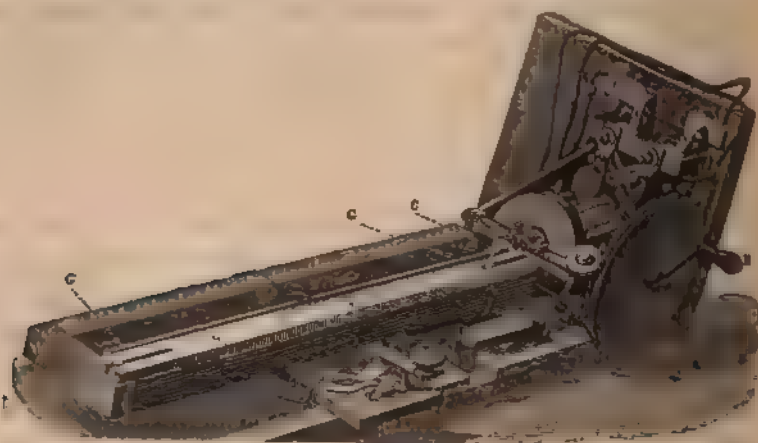


FIG. 5. EXPERIMENTAL SEISMO-METER.

corrugated steel, which fly out more and more, under the action of centrifugal force, as the box is rotated faster and faster. A stout glass tube, *G*, of comparatively small bore, open at both ends, is cemented into a collar in the axis of the box, and rotates

with the box. The box is completely filled with mercury, and the tube partially, hence when the volume of the box expands as its sides fly out the length of the column of mercury in the tube diminishes,

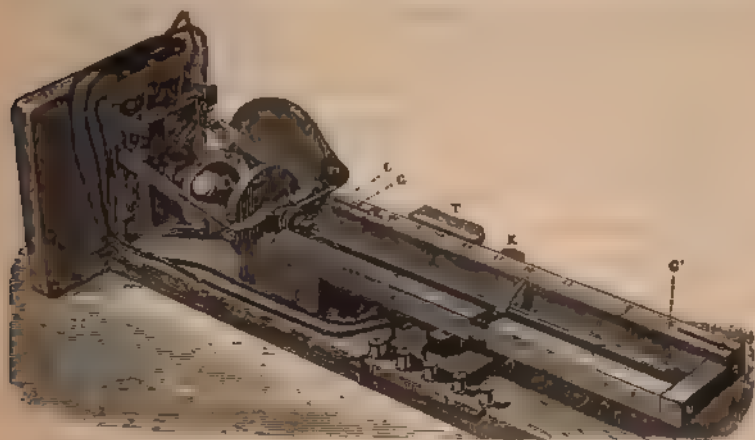


FIG. 5. EXPERIMENTAL SECOHMMETER.

consequently the length of the column at any moment is a measure of the speed of rotation of the box. In the neck of the collar, *C*, in which the tube is cemented, there is a steel tap attached to an axial spindle passing through a tube inside the box, and projected out of this tube at the other end of the box. If this spindle be turned relatively to the box, the tap is opened or closed. At the commencement of an experiment the tap is opened, and the handle, *H*, is turned with the right hand, faster and faster, until, on depressing the key, *K*, with the left hand from time to time, the galvanometer needle is seen to be approaching zero, or the spot of light the zero position on the scale. The key may now be kept depressed, and on turning the handle a little faster a speed is at length reached producing exact balance,—if the handle be turned faster, the needle or spot of light deflects to one side of the zero, if more slowly to the other,—at this moment the trigger, *T*, is lightly touched with the left hand and a spring is liberated. This has the effect of producing a resistance to the rotation of the tap-spindle, which previously was rotating freely with the rotating box, and the tap is thus turned off, cutting off the connection between the mercury in the glass tube and that in the box.

Consequently the mercury in the tube remains, even after the instrument is stopped, of exactly the same length that it had when the trigger was touched. The position of the end of thread of mercury in the tube is now read off on the scale attached, and the apparent increase of resistance of the coil, electro-magnet, or whatever it may be, divided by the number on the scale, gives the required coefficient of self-induction in second-ohms without any further calculation.

The instrument is, therefore, *direct reading*.

This first form of recording speed-indicator was designed to be used vertically, as may be seen from the figure, but we soon found that great advantages were to be gained by placing it *horizontal*. If the glass tube be vertical the mercury in the tube exerts a pressure on the elastic sides of the box, which diminishes as the mercury falls on the speed of rotation being increased. This diminution of pressure causes the mercury not to fall so much for a given speed as it would were there no variation in the pressure due to the mercury in the tube, and so it diminishes the sensibility. Consequently we were led to place the apparatus horizontal. Further, since the mercury in the tube exerts no pressure on the elastic sides of the box, the amount of mercury in the tube does not in any way affect the sensibility, provided that the scale is so placed that the zero is opposite the end of the thread of mercury when the box is at rest. Hence, should any spilling of the mercury by accident occur when the instrument is moved about, the accuracy of the readings is not altered. So that by placing the tube horizontal we obtain greater sensibility and constancy in the readings.

Variations in the temperature from day to day cause alterations in the volume of the mercury, and so alter the length of the thread of mercury in the tube, but this also introduces no error in the experimental result, nor difficulty in using the instrument, since before the commencement of the experiment the scale is slightly moved until the zero on it comes opposite the position occupied by the end of the thread of mercury on the day in question when the box is at rest.

At first, rotating commutators similar to those shown in Fig. 1

were employed with the apparatus shown in Fig. 5; next the brushes were made of a variety of different forms, so as to press *radially* on the rotating commutators to prevent the wear altering the lead and thus changing the sensibility of the instrument; but this form of commutator has at length been entirely superseded by the two oscillating arms, or brushes, *A, A*, worked by a cam. Each arm is composed of several pieces of hard copper, contact being made through the ends, as in many of the switches now used for electric-light work. The end of each brush alternately rubs on a flat piece of phosphor bronze, *P, P*, when it makes contact, and on a flat piece of glass or agate, *g*, when it does not. This form of commutator we found superior for our purpose to the double cylindrical one, since, with the two oscillating arms, the lead can be more easily varied for adjustment; and this slight adjustment of the lead, we may here mention, forms the fine adjustment in the construction of this direct-reading instrument. Further, the slow wearing of this form of brush does not alter the lead, consequently the value of the graduations of the scale remains constant.

The lead aimed at in the construction of these instruments is  $\frac{1}{2}$ , as that gives an equal time to each operation, but in any actual instrument it may be made a little more or less than  $\frac{1}{2}$ , being slightly altered, as just explained, in the final adjusting of the instrument by the maker.

It might appear, at first sight, that the two ends of an oscillating lever, or the two prongs of a vibrating tuning-fork, would perform the required makes and breaks in the right order, but a reference to the cycle of operations given in the table, page 300, and a little consideration, will show that the two prongs of a tuning-fork cannot perform the required cycle of operations in the correct order.

Following the precedent of naming an instrument after the name of the unit employed,—for example, “ammeter,” “voltmeter,” “ohmmeter,” “wattmeter,”—it seems desirable to call this instrument after the name of the commercial unit of self and mutual induction. The absolute electromagnetic unit of self and mutual induction is one centimètre, a name used by all scientific nations.



But the commercial unit of self and mutual induction is  $99,777 \times 10^4$  centimètres, or the second-ohm, which is about 2.3 in a thousand less than  $10^9$  centimètres, or one earth's quadrant. Now in spite of the difference between these two numbers, which, although small, it is a pity to lose sight of, the English word "quadrant" is not used in French, therefore it would not be well to suggest this word as the international name for the unit. Yet it is most important that some name should be universally adopted, since the use of simple familiar names has much to do with making people familiar with the laws of the effect measured by the unit. The unit of electrostatic capacity, the farad, has been called after the greatest experimental worker in electricity; it would therefore seem appropriate that the unit of electromagnetic capacity should be called after Maxwell, the greatest mathematical worker in electricity. We do not, however, like to propose this, as we feel there might be difficulty in obtaining the general acceptance of the name of an Englishman, however great, unless it were sanctioned by an international electrical congress, or unless the man's name was intimately associated in men's minds with self and mutual induction. And Maxwell's large contribution to the subject of electromagnetic induction is surrounded by his equally large contributions to all other branches of electricity and magnetism—a giant surrounded by giants is not prominent. Coming to the last two years, we are glad that the leader and all those who have followed him in taking part in the widening of our ideas on self-induction are still with us. Hence we are driven to suggesting a temporary name for the unit, and as the first three letters in "second" are common to the name in English, French, German, Italian, etc., and ohm is also common, we venture to suggest "*secohm*" as a provisional name, and our instrument we will therefore call a "*Secohmmeter*."

Unless the glass tube in the Secohmmeter just described be rather long, either the sensibility or the range of the instrument must be limited; but a very long straight tube would make the instrument inconveniently large, and a rapidly rotating spiral tube would probably break from centrifugal force acting on these parts of the tube that were not on the axis of rotation. Hence,



the latest form of Secohmmeter (Fig. 6) we have been led to

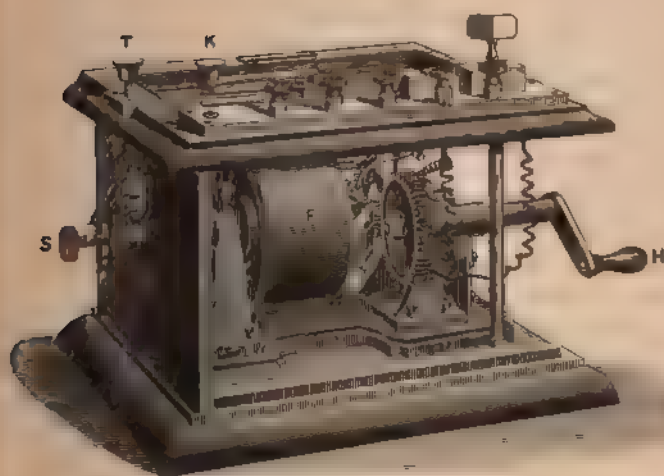


FIG. 6. IMPROVED SECOHMMETER.

employ a *stationary* spiral glass tube, *G*, with its end cemented into a stationary hollow steel conical plug fitting mercury-tight in the collar of the rotating metal box, *B*, with its

weighted elastic sides. This arrangement simplifies the tap mechanism, *T*, seen to the left of the second figure in Fig. 6, as the tap now is not rotating, also many small improvements have been introduced into this last form: for example, at all the joints there is mercury under pressure, so that there is no tendency for air to be drawn into the apparatus at the joints, a fault which sometimes occurred with the earlier form of the apparatus, and led to irregularities in the readings from a bubble of air in the box acting as an air spring, or from air in the glass tube altering the length of the thread of mercury. The temperature adjustment in this last form of the Secohmmeter is made by screwing the screw, *S*, in or out, which slightly alters the volume of the stationary portion of the mercury vessel. The fly-wheel, *F*, has been made to have a much larger moment of inertia, and the box, *B*, is placed inside it so as to be screened from accidental damage.

As both the wires from the battery and the galvanometer come to the Secohmmeter, it is provided with a bridge key, so that the test of the ordinary resistance of the coil, electro-magnet, etc., can be made by using the same key as is used in putting on the battery when the self-induction experiment is being made. When the bridge key is being used in the ordinary way to first close the battery and then the galvanometer circuit, it is obviously necessary that the battery circuit should not be permanently broken nor the galvanometer short-circuited at the commutators of the Secohmmeter, which may by chance have been left after the last experiment in a position which does one or both of these things.

Hence, when using the first form of the apparatus (Fig. 1.) we were compelled to always make sure that the commutating arrangement was put in the right position when at rest before making the ordinary bridge test. Finding this, however, rather troublesome, the key in the instrument (Fig. 5), is made in the following special way:—In addition to the knob, *K*, and the top-spring being able to be depressed, as on an ordinary bridge key, it can be moved sideways, and a cam and spring cause it to remain in one or other of two definite positions. When turned to the left, the commutators are cut out altogether, and

the key acts as a simple bridge key, so that on being depressed it closes the battery circuit and then the galvanometer circuit; but when turned to the right, the commutators are introduced, the one into the battery circuit, the other into the galvanometer short circuit, and the key now simply enables the battery circuit to be closed now and then, or kept permanently closed when balance is being obtained during the self-induction test.

In the last form of the instrument (Fig. 6), with the stationary spiral glass tube, the knob of the key has no side motion, and the connection of the commutating arrangement with the bridge or its disconnection from the bridge is effected by an auxiliary plug key, *p*; but Messrs. Nalder Bros., the makers of the Secohmmeter, are rather in favour of a simple means of carrying out the method, which we used with the first form of the apparatus, of placing the commutating arrangement on the right position when at rest, and using an ordinary bridge key only.

#### MEASURING VERY SMALL COEFFICIENTS OF SELF-INDUCTION.

The first form of the Secohmmeter was used during the autumn of last year for the measurement of rather small coefficients of self-induction which would be very difficult to measure by any other method, such as that of a straight piece of iron wire about 4 mètres long, 3 millimètres in diameter, and having a resistance of 0.1 ohm. For such experiments a metre bridge was employed, *p* + *q*, Fig. 2, being in this case the resistance of 100 centimètres of uniform platinoid wire, *q* being the resistance of *m* centimètres of this wire, and *r* the resistance of a German silver wire having a value of about 0.1 ohm. The following are a sample of the results obtained for the coefficient of self-induction of this iron wire referred to above:—

Revolutions per Minute.	for Balance.	Value of <i>L</i> .
0	50.00	
1200	50.55	0.0000244 sec ohms
0	49.97	
1620	50.70	0.0000247 "
1960	50.85	0.0000252 "
2750	51.20	0.0000257 "
0	49.90	

## MUTUAL INDUCTION.

After the instrument had been in use for some months for the absolute measurement of coefficients of self-induction, it occurred to us that just as the self-induction of a coil,  $S$ , could be made to act as if the resistance of the coil was increased by a definite constant amount, so the mutual induction of one coil,  $B$ , on another,  $S$ , could be made to act as if the resistance of  $S$  were increased by another definite amount the measurement of which would enable the coefficient of mutual induction to be determined by a *cumulative* test. Our idea was to place the coil  $S$  in one of the arms of the bridge, and first determine its coefficient of self-induction in the way already explained, then, in addition, to place the coil  $B$  in the battery circuit, and determine the change in the coefficient of self-induction of  $S$ , which would be of course due to the mutual induction of  $B$  on  $S$ . And the following rather long mathematical investigation, which Mr. Sumpner was good enough to carry out, at our suggestion, shows that this method of experimenting, as we anticipated, leads to an extremely simple formula for the measurement of the coefficient of mutual induction.

Fig. 7 differs only from Fig. 2 in that a coil of resistance,  $b$ ,

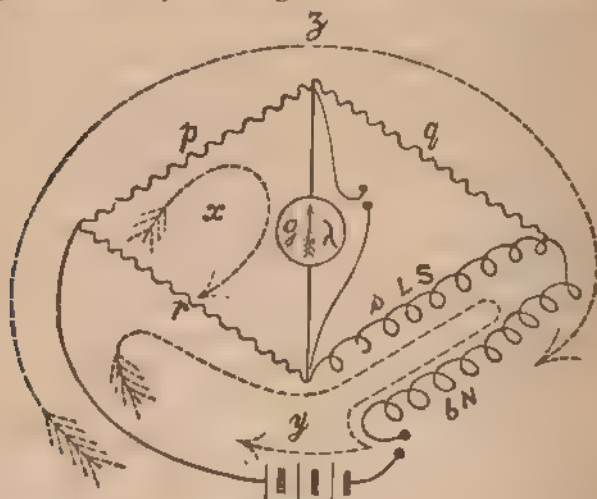


FIG. 7.

and coefficient of self-induction,  $N$ , has been introduced into the

battery circuit, in addition to the coil of resistance,  $s$ , and coefficient of self-induction,  $L$ , which, as before, is in one arm of the bridge.

Let  $M$  be the coefficient of mutual induction of the coils on one another, then, if the battery circuit be closed and the galvanometer not short-circuited, the following equations hold true:—

$$p(x+z) + qx + \lambda \frac{dx}{dt} + r(x-y) = 0 \quad \dots (6)$$

$$p(x+z) + qz + b(y+z) + N \frac{d(y+z)}{dt} + M \frac{dy}{dt} = E (7)$$

$$r(y-x) + sy + b(y+z) + (L+M) \frac{dy}{dt} + (M+N) \frac{d(y+z)}{dt} = E (8)$$

Eliminating  $y$  and  $z$  from these equations, we get

$$Ax + B \frac{dx}{dt} + C \frac{d^2x}{dt^2} + D \frac{d^3x}{dt^3} = E p \sigma \quad \dots \dots (9)$$

where  $A$ ,  $B$ ,  $C$ , and  $D$  are functions of the resistances and coefficients of self and mutual induction, such that

$$A + Bu + Cu^2 + Du^3$$

is identically equal to the determinant

$$\begin{vmatrix} (p+g+r+\lambda u) & p & -r \\ p & (p+q+b+N u) & b+(M+N) u \\ -r & b+(M+N) u & b+r+s+(L+N+2M) u \end{vmatrix} \dots (10)$$

and where  $\sigma$  is the apparent increase of the resistance of the coil  $S$ , as determined by the change necessary to be made to obtain balance when the Secohmmeter is rotated at a certain speed, balance having been previously obtained when the Secohmmeter was at rest; or, in other words,

$$p(s+\sigma) = qr$$

When the Secohmmeter is being rotated at this certain speed.

Then, in accordance with the plan adopted in the solution of the original problem (see page 303), we may divide the effect on the galvanometer into two portions, the one produced from the closing of the battery circuit to the short-circuiting of the galvanometer, occupying a time  $T$ , the other taking place during the short-circuiting of the galvanometer, occupying a time  $T''-T$ . If  $Q_1$  be the quantity of electricity that passes through the galvanometer in the first interval,  $Q_1$  may be obtained from the

following equation obtained from integrating (9) from  $t$  equal nought to  $t$  equal  $T$ :—

$$A Q_1 + B(x_T - x_0) + C \left\{ \frac{dx}{dt_T} - \frac{dx}{dt_0} \right\} + D \left\{ \frac{d^2x}{dt_T^2} - \frac{d^2x}{dt_0^2} \right\} = E p \sigma T \quad (11)$$

where the suffix  $T$  or  $0$  after any expression decides the value to be given to  $t$  in the expression.

Following a similar train of reasoning to that employed in the original investigation, we find from equations (6), (7), and (8) that

$$\begin{aligned} x_0 &= 0, \\ y_0 &= 0, \\ z_0 &= 0, \\ \frac{dx}{dt_0} &= 0 \dots \dots \dots \quad (12) \end{aligned}$$

$$N \frac{dz}{dt_0} + (M + N) \frac{dy}{dt_0} = E,$$

$$(M + N) \frac{dz}{dt_0} + (L + N + 2M) \frac{dy}{dt_0} = E,$$

and by differentiating (6) and using (12) we have

$$p \frac{dz}{dt_0} + \lambda \frac{d^2x}{dt_0^2} - r \frac{dy}{dt_0} = 0.$$

From these we can deduce that

$$-\lambda \left[ \frac{N}{M + N} \frac{M + N}{L + N + 2M} \right] \frac{d^2x}{dt_0^2} = \{p(M + N) + rM\} E.$$

This, by (10), is equivalent to

$$-D \frac{d^2x}{dt_0^2} = \{p(M + N) + rM\} E.$$

We have next to consider the values of the expressions when  $t$  equals  $T$ , which time we assume to be long enough for the currents to become steady, therefore

$$x_T = \frac{E p \sigma}{A},$$

$$\frac{dx}{dt_T} = 0,$$

and

$$\frac{d^2x}{dt_T^2} = 0,$$

so that, substituting the values of the various expressions in (11), we have

$$A Q_1 + \frac{B}{A} E p \sigma + \left( L + \frac{p + r}{p} M \right) E p = E p \sigma T \dots \quad (13)$$

At the time  $T$  the galvanometer is short-circuited and the current in the galvanometer dies away, following the law

$$g x + \lambda \frac{d x}{d t} = 0;$$

so that if  $Q_2$  is the quantity that passes through the galvanometer from  $t$  equals  $T$  to  $t$  equals  $T'$ ,  $T' - T$  being the time during which the galvanometer is short-circuited,

$$g Q_2 + \lambda (x_{T'} - x_T) = 0;$$

$$\text{and } x_{T'} = \frac{E p \sigma}{A},$$

$$\text{also } x_T = 0,$$

since, by hypothesis,  $T' - T$  is long enough for the current to die away in the galvanometer,

$$\therefore Q_2 = \frac{E p \sigma}{A} \cdot \frac{\lambda}{g}.$$

And since for no deflection of the galvanometer needle we must have

$$Q_1 + Q_2 = 0,$$

it follows that  $A Q_1 = -E p \sigma \frac{\lambda}{g},$

and equation (13) reduces itself simply to

$$L + \frac{p+r}{p} M = \sigma \left( T + \frac{\lambda}{g} - \frac{B}{A} \right) \dots \dots \dots (14)$$

an equation only differing from equation (4), the result obtained in the original investigation, by the addition of the term

$$\frac{p+r}{p} M,$$

depending on the coefficient of mutual induction. And since, as will be shown later on,  $\frac{\lambda}{g} - \frac{B}{A}$  is small compared with  $T$ , just as it was in the original investigation, we have approximately

$$L + \frac{p+r}{p} M = \sigma T \text{ secohms.}$$

which, as before, may be written as

$$L + \frac{p+r}{p} M = \frac{60 l}{N} \sigma \text{ secohms} \dots \dots \dots (15)$$

where  $l$  is the lead in the Secohmmeter, and  $N$  the number of revolutions per minute which cause an apparent increase of resistance  $\sigma$  ohms of the coil  $S$ .



To make, therefore, the complete test for determining the coefficient of self-induction,  $L$ , of the coil  $S$ , and the coefficient of mutual induction,  $M$ , between it and any other coil, we first exclude the other coil from the battery circuit, as shown in Fig. 2, and determine  $L$  in the manner already described. We next include the other coil in the battery circuit, as shown in Fig. 7, and repeat the experiment with this Secohmmeter, then

$$M = \frac{p}{p+r} \left( 60 \frac{l}{N} \sigma - L \right),$$

or if  $N_1$  and  $\sigma_1$  are the speeds and apparent increase of resistance in a first experiment, and  $N_2$  and  $\sigma_2$  in a second, we have

$$L = 60 l \frac{\sigma_1}{N_1}$$

$$M = \frac{p}{p+r} 60 l \left( \frac{\sigma_2}{N_2} - \frac{\sigma_1}{N_1} \right) \quad \dots \quad (16)$$

We have stated above that  $\frac{\lambda}{g} - \frac{B}{A}$  is generally small compared with  $T$ , and to ascertain the value of  $\frac{B}{A}$  in (14), in order to investigate this, we may write

$$\frac{B}{A} = \frac{B_1}{A} \lambda + \frac{B_2}{A} L + \frac{B_3}{A} M + \frac{B_4}{A} N;$$

then it is easy to show that

$$\frac{A}{B_1} = g + \text{resistance of network between the extremities of } g,$$

supposing  $g$  removed,

$$\frac{A}{B_2} = s + \text{resistance of network between the extremities of } s,$$

supposing  $s$  removed,

$$\frac{A}{B_3} = b + \text{resistance of network between the extremities of } b,$$

supposing  $b$  removed,

$$\frac{A}{B_4} = \frac{1}{2} \text{ the ratio of the value of an E.M.F. inserted in the branch, } s, \text{ to the steady current set up in the battery branch, } b, \text{ by this E.M.F. in } s.$$

Writing  $r_g$ ,  $r_s$ ,  $r_b$  for the three resistances represented by  $\frac{A}{B_1}$ ,  $\frac{A}{B_2}$ , and  $\frac{A}{B_3}$  respectively, and  $r_b$  for the ratio of the E.M.F. in  $s$  to the current produced by it in  $b$ , equation (14) becomes

$$L + \frac{P + r}{P} M = \sigma \left( T + \frac{\lambda}{g} - \frac{\lambda}{r_g} - \frac{L}{r_s} - \frac{N}{r_b} - \frac{2M}{r_m} \right) \quad (17)$$

If  $g$  is as large or larger than the resistances of any one of the arms of the bridge,  $\frac{\lambda}{g} - \frac{\lambda}{r_g}$  will be very small.  $\frac{\sigma}{r_s}$  is generally small compared with unity, and therefore  $\frac{\sigma}{r_s} L$  can be neglected in comparison with  $L$ , so can  $\frac{2\sigma}{r_m} M$  in comparison with  $\frac{P + r}{P} M$ .

There remains the term  $\frac{\sigma}{r_b} N$ . If  $N$  be very large, then will  $b$  also be very large, and as  $r_b$  is much larger than  $b$ , while  $\sigma$  will be much less than  $b$  if  $b$  be large, it follows that the term  $\frac{\sigma}{r_b} N$  can also generally be neglected; hence the approximate formula (15) will usually be accurate enough for practical purposes. And the value of any one of the last three fractions in the bracket in (17) can be made as small as we like, by adding resistance without self-induction to  $s$  or to  $b$ , a precaution that it is desirable to adopt if it be feared that any one of these three fractions is not negligible in comparison with  $T$ .

### CAPACITY.

Mr. Sumpner has suggested that if, instead of placing a coil with self-induction in one of the arms of the bridge, the arm be shunted with a condenser, there will be an apparent diminution of the resistance of that arm, since such a resistance and condenser in parallel acts like a negative self-induction; and by following the train of reasoning given in this paper it is easy to show that *this apparent diminution, divided by the product of the square of the actual resistance of the arm into the reading of the Secohmmeter corresponding with the speed at which balance is obtained, gives the capacity of the condenser absolutely in farads.*

This formula is far simpler than the one given by Clerk Maxwell for the value of the capacity of a condenser determined by placing the condenser in one of the arms of a bridge and rapidly alternating the connections of the condenser with the bridge.

## APPENDIX I.

## OTHER METHODS OF MEASURING A COEFFICIENT OF SELF-INDUCTION.

As mentioned in the early part of this paper, a number of methods were tried by Mr Sumpner for measuring self-induction, and the appropriate formulæ worked out by him in each case. The following short account of them is given as evidence of our desire not to publish the Secohmmeter method until we had satisfied ourselves that there was not a better one. Indeed, that is one of the reasons why the publication has been delayed so long after the instrument has been in practical use.

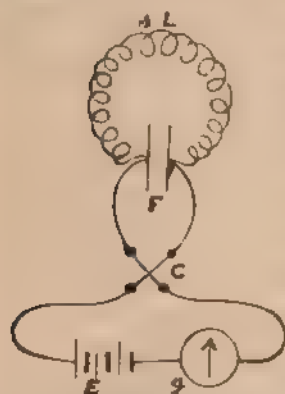


FIG. 8.

Connect the coil of resistance,  $r$  ohms, and a coefficient of self-induction,  $L$  secohms, with the terminals of a condenser of  $F$  farads capacity (Fig. 8). By means of a reversing commutator,  $C$ , alternate the connections of the coil and condenser at the rate of  $N$  times per minute with the terminals of the circuit consisting of the battery of E.M.F. equal to  $E$  volts and a galvanometer, the resistance of which, together with that of the battery, is  $g$  ohms. The object of using the condenser is to prevent the self-induction of

the coil producing sparks on reversing.

Let  $D_0$  be the galvanometer deflection when the commutator is at rest, and  $D_1$  when it is revolving, then it can be shown that

$$L = Fg^2 + \frac{g + s}{2} \cdot \frac{D_0 - D_1}{N D_0} \text{ secohms,}$$

if the reversals of currents in the coil be not too rapid for the currents to reach their steady value, and if the reversing commutator reverses instantaneously without short-circuiting. But this method seems impracticable, as it is impossible to devise a coil

mutator that will reverse without short-circuiting or producing discontinuity for a certain time between every reversal.

If there be short-circuiting, then the condenser may be disengaged with, and if the time of short-circuiting be small compared with  $\frac{L}{s}$ , and if  $g$  be not small compared with  $s$ , it can be shown that

$$L = \frac{g + s}{2N} \left( \frac{D_2 - D_1}{D_0} + m \right),$$

where  $m$  is a very small constant depending on the values of  $g$  and  $r$  and on the proportion of periodic time of commutation during which the short circuit lasts.  $m$  may be eliminated by taking two readings,  $D_1$  and  $D_2$ , at different speeds,  $N_1$  and  $N_2$ , and if the E.M.F. used when the commutator be revolving be  $n$  times as great as when the commutator is at rest, we have

$$L = \frac{g + s}{2D_0} \cdot \frac{D_2 - D_1}{N_2 - N_1} \cdot \frac{1}{n}.$$

This method of testing, although fairly simple, and possessing the great advantage of making the effects *cumulative*, is not nearly as sensitive as the following *zero cumulative* method, which is based on the same principle.

The coil with self-induction condenser and commutator are put in one of the arms of a Wheatstone's bridge, as shown

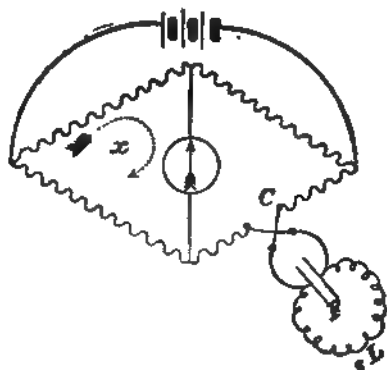


FIG. 9.

In Fig. 9, then with the same notation as before it can be shown, if

- (1st) the speed of reversal be not too great for the currents to reach the steady value,

(2nd) the commutator reverses instantaneously without short-circuiting,  
that

$$L = F s^2 + \frac{1}{1 + \frac{\sigma}{r_s}} \cdot \frac{T \sigma}{2},$$

where  $\sigma$  is the apparent increase of resistance,  $r_s$  the resistance of  $s$  plus that of the network between the extremities of  $s$  supposing  $s$  removed, and  $T$  is the time of a reversal.

It is easy to arrange that  $\frac{\sigma}{r_s}$  shall be negligible, and, unless  $F$  or  $s$  be large,  $F s^2$  can also be neglected, so that we obtain the very simple formula

$$L = \frac{T \sigma}{2},$$

a result that is independent of  $\lambda$ , the coefficient of self-induction of the galvanometer.

To satisfy condition 2 is extremely difficult, if not impossible, and therefore it is better to allow the commutator to introduce a short circuit between the reversals, in which case the condenser previously employed to prevent sparking at the reversals may be dispensed with.

As the complete investigations previously made on the use of the Secohmmeter for the measurement of the coefficients of self and mutual induction have shown that self-induction of the galvanometer does not seriously affect the practical result, we will, for simplicity, in this investigation of the theory of the zero short-circuiting commutator test, neglect the self-induction of the galvanometer from the beginning. Consequently the current  $x$  through the galvanometer (Fig. 9) is given by the equation

$$A x + B' L \frac{dx}{dt} = E p \sigma.$$

Now, if  $Q_1$  be the quantity of electricity that flows through the galvanometer during the reversal, excluding the period when the commutator is short-circuited, and  $Q_2$  the quantity that flows through during this period, and if  $T$  and  $\tau$  be the times of a complete reversal and of a short circuit respectively, reckoned from the commencement of the short-circuiting,

$$A Q_1 + B' L \left( x_T - x_\tau \right) = E p s (T - \tau);$$

also

$$Q_2 = \frac{E p (s + \sigma)}{A'} \tau,$$

where  $A'$  is the value of  $A$  when  $s$  is made equal to nought. If the time of short-circuiting be but a small portion of the whole time of a reversal,  $Q_2$  will be but a small correction. And an examination of the value of  $A$ , given on page 303, will show that unless  $s$  be very large compared with  $b + r$  or with  $g + q$ , putting  $s$  equal to nought will not much change the value of  $A$ ; in other words,  $A'$  may be taken as equal to  $A$ . Hence we have

$$A (Q_1 + Q_2) + B' L \left( x_T - x_\tau \right) = E p (\sigma T + s \tau).$$

Now  $Q_1 + Q_2$  must equal nought for balance, and  $\frac{\tau}{T}$  is a constant, I say, for a given adjustment of a given commutator. Hence

$$B' L \left( x_\tau - x_T \right) = - E p T (\sigma + l s) \dots \dots (18)$$

If  $y_s$  be the *steady* value of the current in  $s$ , then taking into account the partial dying away of the current during the time of short circuit,  $\tau$ , the current in  $s$  immediately after reversal will be .

$$- y_s e^{-\frac{s \tau}{L}},$$

and if  $\tau$  be small compared with  $\frac{s}{L}$ , this is equal to

$$- y_s \left( 1 - \frac{s}{L} \tau \right).$$

From equations (6) and (7), page 325, we can deduce that

$$B' x + E p = B'' y,$$

where  $B'$  or  $B''$  are functions of the resistances; therefore

$$B' x_\tau + E p = - B'' y_s \left( 1 - \frac{s}{L} \tau \right)$$

$$B' x_T + E p = B'' y_s$$

$$\begin{aligned} \therefore B' \left( x_\tau - x_T \right) &= B'' y_s \left( \frac{s}{L} \tau - 2 \right), \\ &= \left( B' x_T + E p \right) \left( \frac{s}{L} \tau - 2 \right); \end{aligned}$$

and we have seen that

$$B' = \frac{A}{r_s},$$

$$\text{and } z_T = \frac{E p \sigma}{A}$$

$$\therefore B' \left( r - r_T \right) = \left( 1 + \frac{\sigma}{r_s} \right) \left( \frac{s}{L} r - 2 \right).$$

Consequently, substituting in equation (18), we have

$$2 L \left( 1 + \frac{\sigma}{r_s} \right) \left( 1 - \frac{s}{2 L} r \right) = T (\sigma + l s).$$

Now  $\frac{\sigma}{r_s}$  will probably be negligible, and

$$s r = s l T,$$

therefore we have  $L = \frac{T}{2} (\sigma + 2 l s);$

hence the effect of the short-circuiting is to diminish the value of  $\sigma$  by  $2 l s$ .

Let two observations at speeds of rotation  $N_1$  and  $N_2$  revolutions per minute be made, giving apparent increases of resistance  $\sigma_1$  and  $\sigma_2$ , then, since

$$T_1 = \frac{60}{2 N_1},$$

and

$$T_2 = \frac{60}{2 N_2},$$

we have

$$L = 15 \frac{\sigma_2 - \sigma_1}{N_2 - N_1}.$$

The following is a sample of the results obtained by using this method with the solenoid previously employed, of which the resistance is about 4 ohms, and the coefficient of self-induction 0.0215 secohms:—



$N$	$s + \sigma$
0	4.13
850	5.20
1030	5.50
1230	5.80
1490	6.20
1650	6.40
1610	6.35
1100	5.57
980	5.4
890	5.3
710	5.0

Plotting these results on squared paper, and determining the value of

$$\frac{\sigma_2 - \sigma_1}{N_2 - N_1}$$

from the mean slope of the line,  $L$  was found to be about 0.0216, a result very near the truth. Subsequent experiments, however, did not give nearly as accurate results, possibly on account of some unknown resistance in the commutator, and that leads to one reason why the method is much inferior to the use of the Secohmmeter, in that the commutator is in one of the arms of the bridge, so that any unknown variation of the resistance of the contacts introduces an error into the test, which, as already explained, cannot happen with the Secohmmeter, as neither of the commutators is in any one of the four arms of the bridge.

This method, therefore, is distinctly inferior to the use of the Secohmmeter.

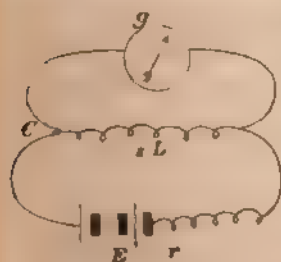


FIG. 10.

Fig. 10 shows another plan tried by Mr. Sumpner, which is cumulative, and although not a zero method, the deflection of the galvanometer from zero is entirely due to the effects of self-induction the coefficient of which

is to be measured; the method, therefore, is fairly sensitive, although inferior of course to a zero method. By means of the commutator  $C$  the coil of resistance  $s$  ohms, and coefficient of self-induction  $L$  is put first in circuit with a battery of E.M.F. equal to  $E$  volts, which may be unknown, and a resistance the value of which together with that of the battery is  $r$  ohms, and next with a galvanometer of resistance  $g$  ohms; the alternation being produced  $N$  times per minute, a speed which must not be too great to prevent the current reaching its steady value each time that the coil is connected with the battery, and to die away in the coil each time that it is connected with the galvanometer. Let  $D_0$  be the galvanometer deflection when the battery and resistance are connected directly in series with the galvanometer, so that

$$D_0 = \frac{b+g}{E},$$

and let  $D_1$  be the mean deflection obtained with the commutator working, then

$$D_1 = \frac{N}{60} \cdot \frac{L}{s+g} \cdot \frac{E}{b+g};$$

$\therefore$  eliminating  $E$ , we have

$$L = \frac{60}{N} \cdot \frac{(s+g)(s+b)}{b+g} \cdot \frac{D_1}{D_0}.$$

To prevent sparking it would be necessary to connect the coil with the galvanometer for a short time before disconnecting it from the battery, and a small correction would have to be applied, depending on the time that the coil was connected with both galvanometer and the battery. This correction takes the form given by the formula

$$L = \frac{60}{N} \cdot \frac{(s+g)(s+b)}{b+g} \cdot \frac{D_1 + l m D_0}{D_0},$$

where  $l$  is the ratio of the time of a short circuit to the time of an alternation, and  $m$  is a numerical function of the resistances, such that when  $b$  is nought  $m$  is equal to  $\frac{g}{s}$ .

By taking two readings,  $D_1$  and  $D_2$ , at speeds  $N_1$  and  $N_2$ , we eliminate  $m$  and obtain

$$L = \frac{60}{n D_0} \cdot \frac{(s + g)(s + h)}{h + g} \frac{D_1 - D_2}{N_1 - N_2},$$

where  $n$  is the ratio of the E.M.F. used when the commutator is revolving to that used when it is at rest.

This method is suitable for employment in cases where only very small direct currents can be employed, as, for instance, in testing the coefficient of self-induction of a high-resistance Thomson's galvanometer without removing the needles. But far greater sensibility is obtained by using the method previously described in which the coil with self-induction is rapidly reversed, since with that method far stronger currents can be sent through the galvanometer, as the effect of rapidly commutating the terminals of the galvanometer, the coefficient of self-induction of which is to be measured, is to send alternating currents through it. These alternating currents do produce a deflection from some cause which is not yet clearly explained, but the deflection is but small, so that the strength of the current that is employed is only limited by the gauge of the wire wound on the galvanometer that is being tested.

## APPENDIX II.

### NON-ZERO USE OF THE SECONIMETER.

1.—In what has preceded we have supposed that the seconimeter was turned at such a speed, and the resistance of the arms of the bridge so adjusted that the galvanometer needle was brought to the zero that it would stand at if no current whatever were passing through the galvanometer. But that is not necessary, for equation (13), page 326, may be written in the form

$$Q_1 = E \frac{(qr - ps) \left( T - \frac{B}{A} \right) - p \left( L + \frac{P + r}{p} M \right)}{A},$$

where  $p$ ,  $q$ , and  $r$  are any resistances in the arms of the bridge (Fig. 7), page 324, and  $s$  the resistance of the coil under test.

$$Q_2 = E \frac{(q r - p s)}{A} \cdot \frac{\lambda}{g};$$

therefore generally the current that will pass through the galvanometer when the secohmmeter is turning at  $n$  revolutions per second is

$$n(Q_1 + Q_2) = \frac{n E}{A} \left\{ (q r - p s) \left( T + \frac{\lambda}{g} - \frac{B}{A} \right) - p \left( L + \frac{p+r}{p} M \right) \right\} \quad (19)$$

producing a deflection  $d$ , say.

If the secohmmeter be stopped and steady currents employed, let balance be obtained by increasing  $s$  to  $s + \sigma_1$ , the values of  $p$ ,  $q$ , and  $r$  remaining as before, and let a deflection  $d'$  be produced when  $\sigma_1$  is changed to  $\sigma_2$ , so that  $d'$  is the deflection produced by a current

$$\frac{E p (\sigma_1 - \sigma_2)}{A}.$$

Hence, since

$$q r - p s = p \sigma_1,$$

we obtain from (19)

$$\frac{d}{d'} = \frac{n}{\sigma_1 - \sigma_2} \left\{ \sigma_1 \left( T + \frac{\lambda}{g} - \frac{B}{A} \right) - \left( L + \frac{p+r}{p} M \right) \right\} \dots \quad (20)$$

Let

$$d \approx l d',$$

where  $l$  is the lead of the secohmmeter: then we have from (20)

$$L + \frac{p+r}{p} M - \frac{l}{n} \sigma_2, \text{ approximately } \dots \dots (21)$$

Now  $\sigma_2$  may be regarded as the apparent increase of resistance of the coil produced by the self-induction on turning the secohmmeter at the rate of  $n$  revolutions per second when the zero used in measuring the resistance with steady currents is at a distance  $d'$  from the true zero, and the zero used in obtaining balance with the secohmmeter revolving is at a distance  $ld'$  from the true zero. This leads us to the following *simplified* method

of measuring  $L$ , the coefficient of self-induction, or  $L + \frac{p+r}{p} M$ , if there be also mutual induction, without its being necessary to *accurately* measure the true resistance of the coil at all:—

1. Let a *rough* approximation be made to the resistance of the coil using steady currents, and let the resistance of the coil

appear to be  $s$  ohms when the spot of light is somewhere near zero, say at 130 divisions from the zero.

2. Let the secohmmeter rotate at  $n$  revolutions per second, and let the coil now have an apparent resistance of  $s + \sigma$  ohms when the spot of light is brought, not to zero, but to  $l \times 130$  divisions on the scale: then

$$L = \frac{\sigma}{n};$$

or

$$L + \frac{p+r}{p} M = \frac{\sigma}{n},$$

if there be also mutual induction.

II.—At the commencement of this paper it was mentioned that we originally measured the coefficient of self-induction not by altering the resistance in one or more of the arms in order to obtain balance, but by leaving the resistances in the arms unaltered, and observing the galvanometer deflection when the secohmmeter was rotated. Equation (19) at once gives the formula that we employed with that method of using the secohmmeter; for, if  $p$ ,  $q$ , and  $r$  be the resistances of the arms that will produce balance with  $s$  for steady currents, equation (19) becomes

$$n(Q_1 + Q_2) = -\frac{n E p}{A} \left( L + \frac{p+r}{p} M \right).$$

Let this current,  $n(Q_1 + Q_2)$ , produce a deflection  $d_1$ ; next let  $s$  be increased by a known resistance,  $s'$ ; and let the galvanometer deflection for steady currents in the arms be  $d_2$ : then this deflection is produced by a current

$$-\frac{E p s'}{A}$$

$$\therefore \frac{d_1}{d_2} = \frac{n}{s'} \left( L + \frac{p+r}{p} M \right);$$

or

$$L + \frac{p+r}{p} M = \frac{d_1}{d_2} \cdot \frac{s'}{n},$$

or

$$L = \frac{d_1}{d_2} \cdot \frac{s'}{n},$$

if there be no mutual induction, the formula originally employed for measuring  $L$ .

This comparative deflection method of using the secohmmeter

has received a high testimonial for great sensibility from MM. P. Ledeboer and G. Mascartier, who, unaware of the fact that it was devised and used by us rather more than a year ago, have recently brought it to notice in France. The large amount of excellent work that M. Ledeboer has done on self and mutual induction makes him well competent to judge of the relative sensibility of methods of measuring the coefficients, therefore we feel sure that he and his colleague will be the first to recognise the great increase of sensibility of our *zero or relative* method beyond even the large sensibility possessed by our *comparative deflection cumulative* method for measuring the coefficients of self and mutual induction.

### USE OF SECCHMETER WITHOUT SPEED-INDICATOR.

III.—It is important to notice that *all* known zero galvanometric methods of comparing the coefficients of self or mutual induction with one another, or with the capacity of a condenser, can be increased *enormously* in sensibility by the use of the secchmeter, and, since in such cases the speed of rotation need not be known, a very simple form of secchmeter without speed-indicator can be employed.

The comparison of the coefficients of self or mutual induction with one another, or with the capacity of a condenser, is usually effected by tests that are completed during the growth or the dying away of a current, since it is only during the variation of a current that self or mutual induction, or the electrostatic capacity of a condenser, evidence themselves. The effect of an error in the balance only lasts for a very short time, and therefore is very small if the error be small; that is, the tests are not sensitive. Resistances are compared with steady currents lasting for a considerable time, but before attempting to accurately measure an unknown resistance it is a common practice to give the bridge key a tap to see whether the balance is so far out that a deflection of the galvanometer is produced for a current lasting for a very short time. Such a preliminary test is intentionally very unsensitive, but its sensibility is equal to that of the ordinary

tests made for comparing the coefficients of self and mutual induction.

But by the use of the secohmmeter it is now possible not merely to measure the coefficients of self and mutual induction and the capacities of condensers *absolutely*, but, in addition, to *secure the same high degree of sensibility with tests that have hitherto had to be completed during the growth or dying away of a current that it is customary to obtain in the use of the Wheatstone bridge for measuring resistances with steady currents.*

The PRESIDENT: I think it very likely that most of us would The President. prefer to have an opportunity of studying the paper a little more before commenting upon it. It is rather a long paper, and requires attentive consideration; but if any member is prepared to make any remarks I shall be very glad to hear them.

Perhaps Dr. Fleming has something to say?

Dr. J. A. FLEMING: I feel sure I shall only be expressing Dr Fleming. what we must all have felt, in saying that this paper of Professors Ayrton and Perry is one of very great interest. It has been for some time a matter of necessity to have an accurate and simple method of determining coefficients of self-induction. This is the more essential now that secondary generators are being largely used. Referring for a moment to previous methods of determining the coefficient of self-induction, it has been generally found that those methods which depend on the use of a condenser are not very convenient. The residual charge and absorption of condensers, especially paraffin paper condensers, introduce practical difficulties. Not along ago the notion occurred to me to vary the bridge method of measuring the induction coefficient in the following manner:—The coil to be measured is made one arm of a bridge, which is then balanced for steady currents. Into the battery circuit is introduced an interruptor, which may be a tuning-fork, breaking the circuit with known frequency. The ordinary galvanometer is then replaced by one of low resistance, and having a needle of soft iron instead of a magnet. Such a galvanometer takes a steady deflection when alternate currents are passed through it, and



Dr.  
Fleming.

accordingly the soft-iron-needle galvanometer indicates a steady deflection when the battery circuit is interrupted rapidly. The value of this indication can be obtained from an independent calibration. I have not put the method yet to very careful test, but hope to be able to do so soon.

I should like to draw the attention of those who are interested in this subject to the very elegant method of determining the mutual induction of two coils which is due to Professor Carey Foster. For the determination of self-induction the method brought before us to-night is certainly very convenient and highly ingenious.

Professor  
Ayrton.

Professor W. E. AYRTON: Before the discussion is continued I should like Mr. Sumpner to give an account of the work he has done in this matter. He has made numerous experiments on the coefficient of mutual induction of a Ferranti machine for all currents passing through it, and for all degrees of magnetisation. He will probably give an account of the results he has obtained. Of course it is indirectly connected with the mode of measuring self-induction described to-night, but I mention it because Dr. Fleming has referred to the fact that the coefficient of self-induction of a magnet is not constant, and Mr. Sumpner can give the results of a Ferranti machine for all degrees of saturation.

In case it should appear that I was slighting Professor Carey Foster in not mentioning his method, might I take this opportunity of saying that I have already at the Physical Society expressed my greatest admiration of his method of measuring mutual induction, and I also mentioned that on the Monday following the Saturday when Professor Foster had published his method at the Physical Society, Mr. Sumpner was at work at it, and made a great number of experiments on mutual induction. I did not mention the method this evening because it was not a method giving a *cumulative* test. I do not mean to say it is not a good method; it is an admirable method; but the object of the paper was to bring before the meeting *cumulative* tests—that is to say, where the effect of a large number of impulses is obtained. I will not criticise Professor Fleming's method, which is a *cumulative* method, now. I will do so later, but, as I mentioned, the

real object of my rising was to ask that you would allow Mr. Sumpner to make his communication before the discussion is closed. Professors  
Ayrton.

The PRESIDENT: We will take Mr. Sumpner's communication at our next meeting, and will then resume the discussion on the paper read this evening; we shall also have a paper by Professors Ayrton and Perry upon running a dynamo with a very short belt. The  
President.

As Professors Ayrton and Perry are both with us this evening, I take the opportunity of asking you to accord them a hearty vote of thanks for their excellent and valuable paper, which must have required much trouble to work out.

The motion was carried unanimously.

A ballot took place, at which the following candidates were elected:—

*Members:*

Sir Thomas Bazley, Bart., M.A. | William Robinson.  
William Sharpey Seaton.

*Associates:*

Patrick Anderson Black. | Matthew Babington Gisborne.  
James E. France. | William Phillips Mendham.

*Student:*

J. Nealer Cooper.

The meeting then adjourned until May 12th.

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The One Hundred and Sixty-seventh Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 12th, 1887—Sir CHARLES T. BRIGHT, M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

It was announced that the Council had approved of the transfer from the class of Associates to that of Members of William H. Stone, of Tokio, Japan.

Donations to the Library were announced as having been received since the last meeting from Sir Frederick Abel, Vice-President; Professor J. A. Fleming; Professor S. P. Thompson; and the Commander F. Salvatori, Local Honorary Secretary for Italy; to all of whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: Our next business is to resume the discussion on Professors Ayrton and Perry's paper on "Modes of Measuring the Coefficients of Mutual and Self Induction," but I will first call on Mr. Sumpner for his communication, as arranged at the last meeting.

## THE MEASUREMENT OF SELF-INDUCTION, MUTUAL INDUCTION, AND CAPACITY.

By W. E. SUMPNER, B.Sc., Associate.

It has long been known in a general way that the effects of self-induction and capacity are opposite in kind. Self-induction in a relay produces sparking, but if a condenser be shunted to the relay sparking is prevented. Condensers are used in induction coils to prevent sparking and to hasten change of magnetisation. Mr. Preece told us while discussing Professor Hughes's paper that a resistance shunted by a condenser is

placed in a telegraph circuit to overcome the effects of self-induction. Professor Silvanus Thompson told us (Journal, May 13, 1886) that Mr. Black in 1878 "showed that one difficulty in long-distance telephoning could be effectually got over by putting "a condenser in the circuit as a bridge to any electro-magnet in "the line." The reason of this is possibly that the condenser diminishes the effective self-induction of the line. Mr. Blakesley remarked on the same occasion that the working of a telephone circuit completed through a condenser could be improved by adding self-induction to the circuit up to a certain amount. After that maximum had been reached, an increase of the self-induction of the circuit or a diminution of its capacity was a disadvantage. Now possibly the reason of this is to be found in the fact that capacity is like a negative self-induction. Capacity in a circuit helps change of current. Self-induction in a circuit hinders change of current. Just as self-induction in a telephone circuit retards some of the waves of current more than others, and thereby renders impure the sounds given out, so capacity in a telephone circuit accelerates some of the waves more than others, and produces a similar result. The ideal telephone circuit should have no effective self-induction. What the resistance of it is does not matter so much. When Mr. Blakesley added self-induction to his telephone circuit, the improvement he noticed in the working was probably not because the mere addition of self-induction was an improvement, but because the effect of the capacity was diminished more and more as self-induction was added, until a point was reached when the effect of the latter counterbalanced that of the former. It was then that the line worked at its best. It was then that the waves of current travelled along the line without changing much in form.

In working with the secammeter it is possible to make the resistance of the coil having self-induction appear either to increase or to decrease, according to the way in which the commutator is turned. It is even possible by turning the secammeter at a particular speed to reduce the apparent resistance of the coil to zero. The reason is that self-induction

when currents are increasing acts like an increase of resistance, and when currents are diminishing like a diminution of resistance. Exactly the reverse holds good of capacity. In the armature of a dynamo self-induction acts like an increase of resistance because the current has to be started in each coil as it passes the commutator. If it were practicable to shunt each coil of the armature by a condenser, the effect of capacity would be that of a diminution of resistance each time the coil passed the commutator. Just as the resistance of an armature is effectively increased by its self-induction, and increased by an amount proportional to its speed, so the result of capacity would be to diminish the effective resistance by an amount proportional to the speed. If, therefore, it were practicable to use condensers of suitable capacity, it would be possible not only to get rid of the effect of self-induction, but actually for a particular speed to diminish the effective resistance of the armature to zero.

It is important to consider whether the opposite natures of self-induction and capacity can be utilised practically in diminishing the self-induction of transformer circuits.

In the appendix of the paper of Professors Ayrton and Perry will be found two or three methods which I devised for measuring self-induction. In each of those methods a condenser, shunted to the coil, was used to prevent sparking at the intervals between the reversals; and although the condenser was afterwards dispensed with because a short circuit was allowed to occur, yet the formula was first worked out in each case on the supposition that the condenser was used. The result came out in every instance just as if the condenser had been absent, and as if the self-induction of the coil had been diminished by the product of the capacity of the condenser and the square of the resistance of the coil shunted to the condenser. Now this not only showed that each of the methods for measuring self-induction was also a method for measuring capacity, but it seemed to indicate that there was some law regulating the external action of a condenser shunted by a wire.

The law was easily found to be as follows:—

In any network of conductors, if one of the branches be com-

posed of a condenser whose terminals are shunted by a wire, with or without self-induction, and if the currents in the network vary from any cause from one steady set of values to another, the extra quantity of electricity which will pass through a galvanometer placed in any arm of the network, in consequence of the capacity, will be exactly as if the condenser were altogether removed and the self-induction of the wire were diminished by the product of the capacity of the condenser and the square of the resistance of the wire shunting it.

Let  $E$  be the electro-motive force, constant or variable, applied to the terminals of a condenser through a resistance  $R$ ; let  $K$  be the capacity of the condenser,  $r$  the resistance, and  $L$  the coefficient of self-induction of the wire shunting the condenser; let  $x$  be the current through the wire  $r$ , and  $y$  the current through the condenser, and let  $V$  be the potential difference of the condenser terminals at any moment: then

$$E - V = R(x + y),$$

$$V = rx + L \frac{dx}{dt},$$

$$y = K \frac{dV}{dt};$$

whence

$$E = (R + r)(x + y) + (L - K r^2) \frac{dx}{dt} - K L r \frac{d^2 x}{dt^2}.$$

The extra current due to  $K$  and  $L$  will be that caused by the extra electro-motive force

$$(L - K r^2) \frac{dx}{dt} - K L r \frac{d^2 x}{dt^2},$$

and the total quantity of electricity passing through a galvanometer placed in any other branch of the network will be proportional to the integral of this extra electro-motive force if the resistances of the branches are not altered. If the currents in the network vary from one steady state to another, the integral is

$$(L - K r^2)(x_1 - x_2),$$

where  $x_1$  and  $x_2$  are the two steady currents through the wire. Now, as these are not affected by  $K$ , the effect on the galvanometer is exactly as if the condenser were removed and the self-induction of the wire were diminished to

$$L - K r^2.$$

The effect of the capacity is that of a negative self-induction. It may be said to be due to this cause that it is possible to compare self-induction with capacity in the way described by Clerk Maxwell. No two quantities can be directly compared when their dimensions are different. In no true comparative method is it necessary to know any quantity absolutely. The fact that Maxwell's method requires the knowledge of resistance absolutely shows that the quantities directly compared are not  $L$  and  $K$ , but  $L$  and  $K r^2$ .

It seems very strange that Clerk Maxwell, who devised a method for measuring self-induction absolutely, and who also gave methods for comparing self-induction, mutual induction, and capacity, failed to observe that his absolute method was equally applicable to the measurement of all these quantities. If a galvanometer be placed in one of the branches of any network of conductors, such as the Wheatstone's bridge, the effect on the galvanometer due to different electro-motive forces acting in different branches of the network is well known to be the sum of the effects produced by each electro-motive force acting separately. Now the effect of self-induction in a wire through which a varying current is passing is to generate an electro-motive force in the wire proportional to the rate at which the current varies. Mutual induction and capacity produce similar effects. So that, while currents are being established in a Wheatstone's bridge in and between the branches of which exist induction and capacity, the extra current through the galvanometer will be that caused by the extra electro-motive forces in the network, and will be the sum of the extra currents produced by each of these electro-motive forces taken separately. The total quantity of electricity going through the galvanometer in consequence of the induction and capacity will be a simple linear function of the coefficients of those quantities.

Indeed, Maxwell has shown that if self-induction exists in one of the arms of a Wheatstone's bridge which is balanced for steady wire's, the swing of the galvanometer when the galvanometer

The closed before the battery circuit, is directly proportional *In* self-induction. He has, moreover, shown that it is possible



to balance the bridge for both steady and variable currents, provided a certain relation holds between the resistances of the arms and the coefficients of induction or capacity that are being compared. If, for example, as in Fig. 5, we are comparing self-induction with capacity, suppose  $L$  is the self-induction of the branch  $r$ , and  $K$  is the capacity shunted to the branch  $q$ , then if  $p s = q r$  and  $K q r = L$ , Clerk Maxwell has shown that the bridge will be balanced for both steady and variable currents. Now this shows that the effect of  $K$  is equal and opposite to that of  $L$ ; and since the swing of the galvanometer when  $K$  is removed is proportional to  $L$ , it follows that the swing of the galvanometer when  $L$  is removed is proportional to  $K$ . The same remark applies to mutual induction. It at once follows that Clerk Maxwell's method of determining self-induction in absolute measure is also a method of determining mutual induction and capacity in absolute measure. We have merely to proceed in exactly the same way as when determining self-induction, and to divide the result we obtain by a simple ratio or a simple product of the resistances in the arms of the bridge.

Perhaps Clerk Maxwell missed seeing this because he foresaw the existence of null methods and did not consider the meaning of the galvanometer swing when the bridge, although balanced for steady currents, was not balanced for variable currents. However this may have been, the null methods of Clerk Maxwell form an example of how very tedious and difficult to work null methods may sometimes become. It is not only necessary to have a very delicate galvanometer and an extremely delicate adjustment of the bridge, but only one particular adjustment of the bridge will give any result at all, and for this reason these double null methods are quite unsuited to those to whom time is any object.

Once, however, the fact is grasped that the swing of the galvanometer in a bridge balanced for steady currents is a linear function of the inductions and capacities in the bridge, it is easy to see a much more rapid way of comparing the coefficients than by the methods of Clerk Maxwell.

Let  $L_x$  be the self-induction of the branch  $x$ ; let  $K_x$  be the

capacity of the condenser shunted to the branch  $x$ ; let  $M_{xy}$  be the mutual induction of the branches  $x$  and  $y$ .

If we are comparing  $L$  with  $M$ , as in Fig. 6, we have

$$L_s + \frac{p+r}{p} M_{ss} = k D_1,$$

where  $p$  and  $r$  are the resistances of the branches as indicated in the figure,  $D_1$  is the first swing of the galvanometer when the bridge being balanced for steady currents has an electro-motive force suddenly put in the battery circuit, and where  $k$  is some constant.

Now reverse the connections of either of the coils possessing mutual induction. The bridge is not altered except that the sign of  $M_{ss}$  is changed. If we now take another swing, we get

$$L_s - \frac{p+r}{p} M_{ss} = k D_2;$$

whence

$$L_s : M_{ss} = \frac{p+r}{p} \frac{D_1 + D_2}{D_1 - D_2}.$$

If we are comparing two self-inductions, as in Fig. 1, we get in the same way, after balancing the bridge,

$$p L_s - r L_q = k D_1.$$

Now remove the branch  $q$  and substitute resistances till the bridge is again balanced. We then have

$$p L_s = k D_2;$$

whence

$$\frac{L_s}{L_q} = \frac{r}{p} \frac{D_2}{D_1 - D_2}.$$

If we are comparing two capacities, as in Fig. 3, which represents a generalised form of De Sauty's method, we have in the same way (if the bridge is balanced for steady currents)

$$s K_s - q K_q = k D_1;$$

by removing the connections of the condenser  $q$  we have

$$s K_s = k D_2,$$

or

$$\frac{K_q}{K_s} = \frac{s}{q} \frac{D_2 - D_1}{D_1} = \frac{r}{p} \frac{D_2 - D_1}{D_1}.$$

This must hold when the resistances  $s$  and  $q$  are infinite—i.e., in De Sauty's method. In this case, however, as well as in the case represented in Fig. 2, the swing method is needless, as a single null method is applicable.

If we are comparing capacity with mutual induction, as in Fig. 4, we get in the same way

$$\frac{q+s}{q} M_{rs} - q r K_q = k D_1;$$

and by disconnecting the condenser we get

$$\frac{q+s}{q} M_{rs} = k D_2,$$

or

$$M_{rs} : K_q = \frac{q^2 r}{q+s} \frac{D_2}{D_2 - D_1}.$$

If we are comparing self-induction and capacity, as in Fig. 5, we get

$$L_r - q r K_q = k D_1;$$

and by removing the condenser,

$$L_r = k D_2,$$

$$L_r = \frac{D_2}{D_2 - D_1} q r K_q.$$

$q r$  must generally be large, and it is better to make  $q$  larger than  $r$ .

The following table gives the results of a set of experiments made on an induction coil the ratio between whose coefficients of self and mutual induction was required. The ratio  $r : p$  of the arms of the bridge was varied from 50 to 200, the ratio of the coefficients being about midway between these numbers.

MUTUAL INDUCTION AND SELF-INDUCTION.

Ratio of Arms $r : p$ .	Induction Measured.	Mean Swing.	Sum of Swings.	Ratio of L : M.	Error.
57.10	$L - 58.10 M$ $L + 58.10 M$	209) 565)	774	126.3	+ 0.9
71.77	$L + 72.8 M$ $L - 72.8 M$	601) 164)			
82.18	$L + 83.1 M$ $L - 83.1 M$	595) 125)	730	127.4	- 0.2
100.77	$L + 101.8 M$ $L - 101.8 M$	594) 65)			
127.07	$L - 128.1 M$ $L + 128.1 M$	- 3) 545)	542	126.7	+ 0.4
206.99	$L + 208.0 M$ $L - 208.0 M$	531) - 125)			
			MEAN	127.3	0.6

On looking at the column headed "Sum of Swings," we see that the numbers gradually diminish to the null position. Now these numbers in each case correspond with the constant amount of induction  $2L$ , and must therefore be a measure of the sensibility of the arrangement of the bridge. The striking fact comes out that in this particular case the double null arrangement was almost that of least sensibility. The reason of this is not far to seek. In measuring resistance by the Wheatstone's bridge the sensibility, for the same galvanometer and battery, depends upon the way in which the bridge is arranged. Now the effect of induction or capacity for variable currents is exactly comparable with that of a variable resistance; so that the bridge, although balanced for steady currents, may be regarded as deranged for variable currents, and the effect on the galvanometer will be greatest when the bridge is joined up for the measurement of resistance in the most sensitive way. It by no means follows that the most sensitive arrangement of the bridge will be the particular arrangement necessary for the null position.

It therefore becomes questionable whether the double null arrangement is worth the trouble of getting. Those who invariably prefer null methods make an assumption which is not true. They assume no less than the possession of a galvanometer of infinite sensibility. All they really have is one galvanometer which is the most sensitive at their disposal, and the best arrangement they can have is that which will, with that particular galvanometer, yield the most sensitive results. However, those who have an extremely sensitive galvanometer or an extremely unreliable battery can use this method of swings merely as an approximation to indicate the null position, and may thereby save much time.

There is a more important consequence of the fact that the extra current through the galvanometer is a linear function of the coefficients of induction and capacity. All Maxwell's comparative methods may be turned into absolute methods, and either Maxwell's absolute method, or, still better, the absolute method of Professors Ayrton and Perry, can be applied to each one of them. Figs. 1 to 12 illustrate this.

Fig. 1 represents Maxwell's method of comparing two self-inductions, but we may regard the self-induction in the branch  $q$  as removed, provided we suppose that of the branch  $s$  to be

$$L_s = \frac{r}{p} L_q.$$

Fig. 2 represents Maxwell's method of comparing two mutual inductions. Fig. 11 represents the same method when the current runs through the bridge. For mere comparison it is better to use the arrangement of Fig. 2. For absolute measurement it is better to use the arrangement of Fig. 11. In the latter case we may regard the effective self-induction of the branch  $p$  as being

$$\frac{s+q}{s} M_{ps} \pm \frac{r}{s} \left( \frac{q+s}{s} \right) M_{qs},$$

and we may regard the mutual induction as being altogether removed.

Fig. 3 represents a method of comparing two capacities. We may regard the branch  $s$  as having an effective self-induction of

$$\frac{r}{p} \cdot q^2 K_q - s^2 K_s.$$

Fig. 4 represents a method of comparing mutual induction with capacity. We may regard the effective self-induction of the branch  $r$  as being

$$\pm \frac{s+q}{q} M_{rs} - q r K_q.$$

Fig. 5 represents Maxwell's method of comparing self-induction with capacity. We may regard the effective self-induction of the branch  $r$  as being

$$L_r = q r K_q.$$

Fig. 6 represents Maxwell's method of comparing self-induction with mutual induction. The effective self-induction of the branch  $s$  is

$$L_s \pm \frac{p+r}{p} M_{ps}$$

Figs. 7, 8, 9 represent analogous methods of finding self-induction, mutual induction, and capacity. The effective self-induction of the branch  $s$  is  $L_s$  for Fig. 7,  $\frac{p+r}{p} M_{ps}$  for Fig. 8, and  $-s^2 K_s$  for Fig. 9.

Fig. 10 is an illustration of the combination of the above methods. The effective self-induction of the branch  $s$  in this case is

$$L_s = p s K_p + \frac{s+q}{q} \cdot \frac{q}{p} M_{pq} - \frac{p}{q} L_r$$

The above expressions may be used not only when the bridge is balanced, as in Maxwell's method, but also when the bridge is deranged, as in the method of Professors Ayrton and Perry, provided that the derangement of any arm  $x$  is small compared with  $R_{xx}$ , viz., the resistance of the branch  $x$  together with the resistance of the network between the extremities of the branch  $x$ .

Fig. 12 represents De Sauty's method of comparing capacities. If the null position has not been obtained, the value of the first swing of the galvanometer can be found by the same method that Professor Fleeming Jenkin used, and by this means the difference of the capacities can be found absolutely. We may accumulate by periodically shunting and unshunting the bridge. We cannot do so by making and breaking the battery circuit, since the condensers must be discharged.

The interest of the above methods consists in the fact that they are methods to measure absolutely the difference between two quantities. The most accurate way of measuring the ratio of two resistances is well known to be that of Professor Carey Foster, who measures the difference between two nearly equal resistances in terms of the resistance of an observed length of a uniform wire; and by that means, if either of the resistances is known fairly accurately, their ratio can be determined with extreme accuracy. The above methods are analogous with that of Professor Carey Foster.

When, however, we employ the cumulative method exemplified in the secolimeter of Professors Ayrton and Perry, the analogy is still more complete. In using a wire bridge it is very difficult to determine the exact position of the slider, although it is very easy to determine accurately the difference between two of its positions; and the great accuracy of Professor Foster's method is due to the fact that it is not necessary to determine the absolute position of the slider, because all that is

required is the difference between two positions. Now one of the faults of Maxwell's methods consists in the necessity of having a very exact balance of the bridge. As an example, when the branches  $r$  and  $s$  were each about 6,000 ohms, it was found necessary to adjust  $r$  to a small fraction of an ohm by adjusting the length of a bare wire connecting two terminals, and even then the heating caused by keeping on the battery for a moment so altered the balance that a correction had to be applied. However, when the cumulative method of Professors Ayrton and Perry is employed, it is not necessary to have the bridge balanced at all. It is only necessary to have the bridge so nearly balanced that the spot may be on the scale.

This result is a simple deduction from equation (13) of the paper of Professors Ayrton and Perry. It may also be proved as follows:—

If there is no induction in the bridge, but the arm  $s$  is deranged from balance by an amount  $\sigma$ , the deflection  $D_0$  of the galvanometer will be proportional to  $\sigma$ , for small derangements; so that

$$D_0 = k \sigma,$$

$k$  being a constant. If the battery is on for only a portion ( $l$ ) of the time, the deflection  $D_0^1$  will be  $l$  times this; so that

$$D_0^1 = (l D_0) = l k \sigma;$$

and if we alter the resistances  $r$  and  $s$  by small amounts proportional to  $r$  and  $s$  the deflection will not be altered. Now in testing for self-induction we practically increase the resistance of the arm  $s$  by an amount  $\sigma$  in consequence of the self-induction, and in order to find out what is the value of  $\sigma$  we increase the arm  $r$  by an amount which bears the same proportion to  $p$  that  $\sigma$  does to  $q$ . We shall know this is the case when the deflection of the galvanometer is  $l D_0$ , where  $l$  is the lead of the secohmmeter—i.e., the proportion of the time the galvanometer and battery are on together to the period of the commutator.

So that, in testing with the secohmmeter,

- (1) Observe the position of rest ( $Z$ ) of the spot.
- (2) Adjust the bridge roughly for resistance until the spot comes to a point,  $D_0$ , not far from  $Z$ .



- (3) Take a point on the scale between these two, such that its distance from Z bears to the distance between Z and D, the proportion of the lead of the instrument, and use this point as a zero.
- (4) Derange the bridge by a known amount, and turn the commutator till the spot of light passes slowly through the zero. At this moment press the key and take the speed reading.

By this method only one error of judgment can arise, viz. the determination of the speed.

The analogy with the method of Professor Foster is clearly shown by the fact that the same wire bridge can be used. If in Fig. 7, the resistance  $p + q$  be that of a straight uniform wire, we have—

1. For resistance balance—

$$s : r = q : p,$$

or

$$s : r + s = q : q + p.$$

2. For commutative balance—

$$s + \sigma : r + s + \sigma = q' : q' + p'.$$

where  $\sigma$  is the effective increase of resistance owing to self-induction.

$$\sigma = \frac{n}{l} L_s,$$

$n$  being the number of revolutions per second, and  $l$  the lead.  $q'$  is the new value of  $q$ , and  $q' + p' = q + p$ .

Now, generally,  $\sigma$  is small compared with  $s$ ; so that

$$s : r + s = q' - q : p + q,$$

and

$$L_s = \frac{l}{n} (q' - q) \frac{r + s}{p + q}.$$

The two chief advantages of the cumulative method are—

- (1) Impulses are turned into steady currents as far as galvanometers are concerned, and therefore null methods are applicable which would otherwise be impossible.
- (2) The sensibility of the galvanometer is practically multiplied by the speed of the commutator. The only limit is that given by the ratio between the period of the commutator and the time constant of the

network—i.e., the time needed for currents to completely establish themselves. This renders it possible to much improve any existing null method, however good.

The sensitiveness of any of the above-mentioned difference methods depends upon three considerations—

- (1) The sensitiveness of the arrangement of the bridge for the measurement of resistance.
- (2) The time constant of the bridge.
- (3) The amount of the difference measured.

Thus, in order to determine the ratio between  $L$  and  $M$  accurately by the method of Fig. 6, it is necessary to remember that

$$l - a M = b D + \frac{\sigma}{N},$$

where  $D$  is the deflection caused by a small derangement from the null position, and  $a$  and  $b$  are constants;  $N$  is the speed reading of the screwmeter, and  $\sigma$  the apparent increase of resistance. The most advantageous value of  $b$  will be given by (1) and (2), combined with the consideration that, other things being equal, it is best to have  $a$  as nearly equal to  $L/M$  as possible.

If the comparative methods of Maxwell can be rendered delicate by cumulation, all that is required for practical purposes is done, for we have good standards of capacity, and coils of wire having constant self-induction can easily be made. If, however, the effects are not accumulated, it seems as though no comparative null method of measuring these quantities can be more delicate than would be the measurement of resistance by a Wheatstone's bridge, if it were only possible to touch the battery key for an instant. This process is employed when the bridge is far from balance, in order to prevent the effect on the galvanometer from being too great; and this is the process which must be used in the measurement of induction and capacity, since their effect vanishes after the first instant. Even the comparative method of Professor Carey Foster, excellent as it is, could be much improved by using a cumulative method, not only because the galvanometer is rendered more sensitive, but also because a

slight difficulty which renders a very exact balance impossible could be overcome—viz., unless the galvanometer is very ballistic, the condenser effect asserts itself before the mutual induction effect, and imparts a slight impulse to the needle, which it is difficult to allow for accurately.

If the cumulative method be employed, and the readings for different speeds plotted, the points should lie on a straight line. As the zero point on this line is absolutely known, it is possible to get the slope of the line with very great accuracy.

Maxwell's absolute method being thus equally applicable to the measurement of self-induction, mutual induction, and capacity, it seemed very probable that any method of measuring either of these quantities would suggest methods of measuring the remaining two. For that purpose I have traced out an analogy, which is represented in the figures. The first twelve figures show the result of the analysis of Maxwell's methods. The next six figures (13-18) represent the result of an analysis of Fleeming Jenkin's method of determining the capacity of a condenser. Figs. 13, 14, and 15 represent analogous methods of determining the self-induction of a coil, the mutual induction of two coils, and the capacity of a condenser. The coil or condenser is charged by making the battery connection, and discharged through the galvanometer. In the case of mutual induction the alteration of the current in the battery circuit causes a quantity of electricity to rush through the galvanometer, which is a measure of the product of the change of current and the mutual induction. In each case it is necessary to determine the meaning of the swing of the galvanometer, and to eliminate the unknown electro-motive force of the battery. This can be done by the same method in each case.

If, however, the battery and galvanometer connections are made and broken periodically the effects are accumulated, and the galvanometer will give a steady deflection, which, on being compared with the deflection given by the same galvanometer and battery when a known resistance is in circuit, will render it possible to determine the required coefficient directly in terms of the resistances used, the ratio of two deflections, and the period of

the commutator cycle. A ballistic galvanometer may therefore be dispensed with. In the method indicated by Fig. 13 a short interval may be allowed to occur between breaking the battery connection and making the galvanometer connection, provided a condenser be used. It is, however, better to dispense with a condenser, and allow a short time to elapse between making one connection and breaking the other, for when the coil having self-induction is suddenly isolated its electro-kinetic momentum will cause the potential difference of the condenser shunted to it to alter almost instantly to many times its original value. In either case, however, a correction will have to be applied to the formula. In the method of Fig. 15 an interval may with advantage be allowed to occur between the breaking of one connection and the making of the other. In the method of Fig. 14 it is advantageous to make the connection which short-circuits the galvanometer before breaking the battery connection.

As, however, the effect of accumulating all the impulses in one direction is, as far as galvanometers are concerned, to produce steady currents, it is easy to develop these methods into null methods. We have only to shunt part of the battery current continuously through the galvanometer. Figs. 16, 17, and 18 show the result. The battery has to be placed differently in Figs. 16 and 18, because a condenser discharges in the opposite way to that of a coil having self-induction. A particular case of Fig. 18 is when the resistance  $R$  shunting the condenser is made infinite. If we suppose the self-induction of the wire  $R$  to increase, and the capacity of the condenser to diminish, the effect on the galvanometer due to the induction diminishes through zero till, in the limit when the capacity of the condenser is nothing, we have merely the case of Fig. 16. The remarks about the commutators of Figs. 13, 14, and 15 apply equally to those of Figs. 16, 17, and 18. In Fig. 17 it is seen that the commutator develops into that used by Professors Ayrton and Perry.

In seeking the method of measuring mutual induction analogous to the methods of Figs. 16 and 18, it was found that the end  $X$  (Fig. 17) of the battery branch could be placed at any point on the bridge along  $A D C$ . Now, if the point  $X$  be

connected with D, we have Maxwell's method; while, if it be connected with C, we have a simple deduction from Professor Carey Foster's method when we accumulate, since, under these circumstances, it is easy to see that the condenser used by Professor Carey Foster is needless. There is, consequently, a close relationship between all these methods, and no one of them is distinct.

Figs. 19-21 show methods for finding capacity and self-induction which are analogous with a method given by Maxwell (Vol. II., sec. 776). Fig. 20 represents a Wheatstone's bridge in which one of the arms, A B, is absent, and this absent arm may be supplied by any one of those represented in Figs. 20A to 20F. Figs. 19 and 20A represent Maxwell's method of finding capacity (Vol. II., sec. 776)—a method to which he was led by the consideration that if the connections of a circuit with the terminals of a condenser were periodically interchanged, the effect of the capacity would be comparable with that of a resistance. The same analogy led to a method of finding self-induction described in the appendix of the paper of Professors Ayrton and Perry, and represented in Figs. 20E and 21. The difference between the two cases consists in the fact that the more rapidly a condenser is commutated the less is its apparent resistance, while the reverse is the case with self-induction. In Fig. 20I, if the resistance  $Q$  of the self-induction coil increases without limit, we have simply Maxwell's method, in which it is best to allow an interval between the reversals. If, on the other hand, there is no condenser used, a small short-circuit may be allowed to occur during reversal, and its effect eliminated in the way described in the appendix of the paper of Professors Ayrton and Perry. Professor J. J. Thomson (*Phil. Trans.*, III., 1883) gives a modification of Maxwell's method, which consists of breaking the condenser connection and discharging it, instead of reversing its connections. In Fig. 20C much the same result is brought about by employing a revolving connector, which short-circuits the condenser during a small fraction of a revolution. The formula will have to include a correction for the short-circuiting the arm A B of the bridge while the condenser is being discharged; but this

correction need be but very small, for the time constant of the condenser for discharge, being the product of its capacity and the resistance of the short-circuit, is very small indeed, so that the short-circuiting need only last for a very small fraction of a revolution. Figs. 20B and 20D are but extended cases of Figs. 20A and 20C. Maxwell's method may be regarded as a special case of the method represented in Fig. 20E. Fig. 20F represents a method of finding self-induction somewhat analogous with that of Fig. 20C, but it has not yielded good results. There are analogous methods of determining mutual induction, but they are not very promising. In these methods the commutator is in the arm of the bridge. This is no disadvantage in the case of measuring capacity, but it is a disadvantage in the case of self-induction unless the resistances of the bridge are much larger than that of the commutator. The connection of these methods with the preceding is shown by Fig. 18. If, instead of discharging the condenser through the galvanometer, the condenser be charged through the galvanometer, we get Professor J. J. Thomson's modification of Maxwell's method.

Fig. 22 represents a method suggested by Professor Ayrton for determining the coefficient of self-induction of a coil while a particular current is flowing through it. It is but a slight modification of Maxwell's method, and consists in altering by a small amount the current running through the bridge instead of establishing it from zero to its full value. It can be easily shown under these circumstances that

$$L = \frac{C_1'}{C_1 - C_2} \times \frac{\sin. \frac{1}{2} \theta}{\tan. \alpha} \frac{R T}{\sqrt{\pi^2 + \lambda^2}} e^{\frac{\lambda}{\pi} \tan. \frac{\pi}{\lambda}}$$

where  $C_1'$  is the current running through the coil when a derangement  $R$  of the arm of the bridge in which the coil is placed produces a steady deflection  $\alpha$  on the bridge galvanometer, and  $C_1 - C_2$  is the change of current in this arm which, when the bridge is balanced for steady currents, produces a swing  $\theta$ ;  $T$  is the period and  $\lambda$  the logarithmic decrement of the same galvanometer.

The experiments, which were performed with the aid of

Messrs. Rossiter and Watney, of the Central Institution, were made on the field magnets of a Gramme dynamo, and also on those of a Ferranti dynamo. The arms A B and A D had resistances of about 10,000 ohms each, B B<sub>1</sub> was the coil whose self-induction was required, D D<sub>1</sub> was a German silver wire of about equal resistance, D<sub>1</sub> B<sub>2</sub> was a very thick German silver wire on which a sliding connector (C) was placed. The D'Arsonval galvanometer G<sub>1</sub> connected A and C through a key. The mercury cups B<sub>1</sub>, B<sub>2</sub>, were connected by a short-circuit plug, except when the deflection corresponding to a known derangement R of the arm B C was required. *a a a* are accumulators; R R R is a resistance capable of being inserted more or less into the battery circuit by means of the switch S. The currents were changed by moving the switch, and the change of current was read by a second D'Arsonval galvanometer (G<sub>2</sub>). By pressing the key K<sub>1</sub> the galvanometer G<sub>1</sub> was shunted to a constant length of the wire D<sub>1</sub> B<sub>2</sub>, and measured the current absolutely. By pressing the key K<sub>2</sub> the galvanometer was shunted to any convenient length of the wire D<sub>1</sub> B<sub>2</sub>, and by taking the deflections corresponding with C<sub>1</sub>', C<sub>2</sub>, and C<sub>3</sub> the ratio  $\frac{C_1'}{C_2} = \frac{C_1}{C_2}$  was easily found.

The currents used varied up to 30 ampères, and were sent in both directions. The results show that the coefficient varies with the current, and also with the previous history of the iron. The set of values obtained with an ascending series of currents differs from that obtained with a descending series. The coefficient diminishes rapidly at first, and then more and more slowly. In several cases the results strongly indicate that the maximum value of the coefficient is not that for zero current. This is exactly what it is natural to expect, on the assumption that the self-induction curve is the differential of that of magnetic permeability; for the slope of the latter curve is small at first, increases to a maximum, and then diminishes asymptotically. All the results are, however, much complicated by the fact that the D'Arsonval galvanometer was not sufficiently ballistic. Under these circumstances changing the resistance in the battery circuit alters the time constant of the bridge,



although it does not for the same change of current alter the quantity which passes through the galvanometer. The ratio between the swing of the galvanometer and the quantity passing through it was therefore not constant, and the results are therefore not accurate.

This method is equally applicable to the measurement of change of coefficients of mutual induction and capacity. It is not easy to render it accumulative. If, instead of making and breaking the battery circuit, the points B and D be periodically shunted and unshunted by a known resistance, it will be necessary to know the resistances in the other parts of the bridge; and this will be difficult if the resistances heat, as they must when large currents are used. In the method just described the only resistance necessary to know was that of  $R$ , and this was not allowed to heat.

#### GENERAL FORMULA.

The following method of treating the general problem of a network of conductors will be found simpler in working than that of Clerk-Maxwell, who (Vol. I., sec. 280) expresses potentials in terms of the conductivities of the different branches:—

Let  $E_x$  denote the E.M.F. in any branch  $x$  of the network.

- |            |   |  |   |   |   |
|------------|---|--|---|---|---|
| „ $C_x$    | „ | current  | „ | „ | „ |
| „ $L_x$    | „ | coefficient of self-induction in any branch $x$ of the network.  |   |   |   |
| „ $M_{xy}$ | „ | coefficient of mutual induction between the branches $x$ and $y$ of the network.   |   |   |   |
| „ $x$      | „ | resistance of the branch $x$ .   |   |   |   |
| „ $R_{xy}$ | „ | ratio of a definite electro-motive force in the branch $x$ to the steady current in the branch $y$ produced by it. This ratio depends only on the resistances of the network branches. |   |   |   |

Let the positive direction of any branch be defined as that direction in which an electro-motive force must act in that branch to produce a current in a particular direction in a particular branch, and let the current or E.M.F. of a branch be considered positive when acting in the positive direction of the branch.

It follows, from Maxwell (Vol. I., sec. 281), that

$$R_{xy} = + R_{yx},$$

and also that the above definition is equivalent to the following:—

The positive direction of any branch is the direction of the current produced in it by a positive electro-motive force in a particular branch.

The kinetic energy due to the mutual induction of the branches  $x$  and  $y$  is

$$M_{xy} C_x C_y$$

If the electro-motive forces in the network tend to increase either of the currents  $C_x$  or  $C_y$ , there must be created an opposing electro-motive force in the other branch, since otherwise energy would be created. From this it follows that

$$M_{xy} = + M_{yx}.$$

Since self-induction, mutual induction, and capacity supply extra electro-motive forces while the currents are changing, we can at once write down an expression for the current passing through any particular branch  $g$

$$C_g = \sum \frac{E_x}{R_{gx}} - \sum \frac{L_x}{R_{gx}} \frac{dC_x}{dt} - \sum \sum M_{xy} \left\{ \frac{1}{R_{gy}} \frac{dC_y}{dt} + \frac{1}{R_{yx}} \frac{dC_x}{dt} \right\} \\ + \sum \frac{z^2 K_i}{R_{gi}} \frac{dC_i}{dt} \dots \dots \dots (1)$$

where we suppose each condenser  $K_i$  to be shunted by a wire of resistance  $z$  and no self-induction. Under these circumstances it has been shown in an earlier part of the paper that the extra electro-motive force due to this cause is

$$+ z^2 K_i \frac{dC_i}{dt} = + z \frac{dK_i V_i}{dt} = z \frac{dQ_i}{dt},$$

where  $V_i$  is the potential difference and  $Q_i$  the charge of the condenser  $z$ .

If, with Maxwell, we define the electro-kinetic momentum of the branch  $x$  to be

$$P_x = L_x C_x + \sum M_{xx} C_x,$$

we may write equation (1)

$$C_g = \sum \frac{E_x}{R_{gx}} - \sum \frac{1}{R_{gx}} \frac{dP_x}{dt} + \sum \frac{z}{R_{gi}} \frac{dQ_i}{dt} \dots (2)$$

In order to get the effect of a condenser whose terminals are

merely connected to two points on the network, we shall have to consider the limit of  $\frac{z}{R_{zg}}$  when  $z$  is made infinite.

We get, by integrating (2),

$$Q_g = Q_e - Q_p + Q_t \quad \dots \quad (3)$$

where  $Q_g$  = total quantity which has passed through the galvanometer,

$$Q_e = \sum t \frac{E_x}{R_{zg}},$$

$$Q_p = \sum \frac{1}{R_{zg}} \{ (P_x)_t - (P_x)_o \},$$

$$Q_t = \sum \frac{z}{R_{zg}} \{ (Q_x)_t - (Q_x)_o \},$$

the suffixes  $t$  and  $o$  denoting the time of evaluation. If the currents proceed from one steady state to another, we can evaluate the effect of all condensers shunted by wires by diminishing the self-induction of each wire by  $Kz$ ,  $K$  being the capacity of the condenser and  $z$  the resistance of the wire. In the case when  $z$  is infinite we must find the limit of  $\frac{z}{R_{zg}}$ .

Imagine the terminals of the condenser replaced by those of a battery, and let  $\rho$  be the ratio of the current through the galvanometer to that through the battery: then

$$\rho = \left[ \frac{z}{R_{zg}} \right]_{z=\infty}$$

For imagine an electro-motive force  $E$  to be located in the branch  $z$ . The ratio  $\rho$  is, by definition,

$$\frac{E}{R_{zg}} : \frac{E}{R_{zz}} = \frac{R_{zz}}{R_{zg}}$$

But  $\frac{R_{zz}}{R_{zg}}$  is independent of  $z$ , as we show later (10); so that  $\rho$  is also

$$\left[ \frac{R_{zz}}{R_{zg}} \right]_{z=\infty} = \left[ \frac{z}{R_{zg}} \right]_{z=\infty}$$

because  $R_{zz} = z + R_{oo}$ , where  $R_{oo}$  is the resistance of the network between the extremities of  $z$ , supposing  $z$  removed.

It follows  $\frac{1}{\rho} = (\text{coefficient of } z \text{ in } R_{zg}).$

It is noteworthy that we may write equation (1)

$$C_g = \sum \frac{E_r}{R_{rg}} - \sum \frac{L'_r}{R_{rg}} \frac{dC_r}{dt} + \sum \frac{z}{R_{rg}} \frac{dQ_z}{dt} \quad \dots \quad (4)$$

where

$$L'_r = L_r + \sum \frac{R_{rg}}{R_{rr}} M_{rg};$$

so that the total effect on the galvanometer will be exactly as if the self-induction of every branch  $r$  were increased from  $L_r$  to  $L'_r$ , as above defined, and the mutual induction removed.

Equation (3) shows that for a complete cycle  $Q_p$  and  $Q_z$  will be each zero, provided the resistances of the bridge are not changed. If, however, as in an Ayrton and Perry's cycle, some of the resistances are changed, the cycle must be divided into parts during which no such change takes place, and the integrals for each part must be added. In such cases the self-induction of the galvanometer will probably produce an effect, and must be allowed for.

Thus Mr. H. B. Bourne, while experimenting at the Central Institution on the apparent resistance of a periodic make-and-break placed in one of the arms of the bridge, found that by adding self-induction to the galvanometer branch the result was considerably affected.

The definition given above of the positive direction of a branch serves our present purpose. It is, however, faulty in so far that a positive electro-motive force in any branch does not necessarily produce positive currents in *all* the other branches. The ambiguity arises from the fact that the network functions  $R_{ry}$  are constrained by definition to be essentially positive quantities.

#### GENERAL NETWORK OF CONDUCTORS.

The most general case of a network of conductors is that in which  $p$  points are connected by branches in such a way that each point is the junction of three branches, and of three branches only. Any other network can be deduced from this by diminishing to zero the resistances of one or more of the branches. In the general case  $p$  must be even, since the number of branches is  $\frac{3}{2} p$ . Let  $p = 2n$ , and call  $n$  the degree of the network. If

we increase to infinity the resistance of any branch, we merely diminish the degree of the network by unity.

There will be  $3n$  branches,  $3n$  currents,  $\frac{3n(3n-1)}{2}$  network functions of the form  $R_{xy}$ , and  $3n$  more of the form  $R_{xx}$ . There are only  $(n+1)$  independent currents, since at each of the points of the network we have an equation of continuity, and  $2n-1$  of these are independent.

Moreover, for each point of the network we have three equations of continuity for the network functions—

$$\left. \begin{aligned} \frac{1}{R_{xx}} &= \frac{1}{R_{xy}} + \frac{1}{R_{xz}} \\ \frac{1}{R_{yy}} &= \frac{1}{R_{yx}} + \frac{1}{R_{yz}} \\ \frac{1}{R_{zz}} &= \frac{1}{R_{zx}} + \frac{1}{R_{zy}} \end{aligned} \right\} \dots \dots \dots (5)$$

where the branches  $x, y, z$  meet in a point. We shall get  $(n+1)$  independent equations connecting the electro-motive forces, the currents, and the resistances of the different branches. These equations will be linear for either currents or resistances. By solving for any current  $C_x$  we shall get

$$\Delta C_x = \sum E_y \Delta_{yx},$$

where  $\Delta$  is a determinant of the resistances of degree  $(n+1)$  which will be the same for each of the independent currents, and which, in consequence of the simple sum and difference relation between these currents and the others, will be the same for all the currents.  $\Delta_{yx}$  will be a first minor of  $\Delta$  corresponding with the electro-motive force  $E_y$  and the current  $x$ . Its degree is  $n$ . Suppose the only electro-motive force is that in the branch  $y$ , then, from definition,

$$R_{xy} = \frac{\Delta}{\Delta_{yx}}.$$

Now  $\Delta$  must consist of products formed by taking certain combinations of  $(n+1)$  of the resistances. It can only consist of positive terms; for, from simple physical considerations,  $R_{xx} = x + R_{ox}$ , where  $R_{ox}$  is the resistance of the network between

the extremities of the branch  $x$ , supposing  $x$  removed.  $R_x$  must be positive, and is independent of  $x$ ;

$$\text{where } R_x = \frac{\Delta}{\text{coefficient of } x \text{ in } \Delta} = \frac{\Delta}{\Delta_{xx}} \quad \dots \quad (6)$$

The sign of  $\Delta$  must therefore be the same as that of the coefficient of  $x$ . But  $x$  is any resistance of the network, therefore each term of  $\Delta$  must have the same sign.

This argument also proves that  $\Delta$  can contain no power of  $x$  higher than the first, for otherwise it would involve  $x$ . Hence  $\Delta$  consists of products of different combinations of  $(n+1)$  resistances of the network, and consists of only positive terms. It does not involve all the combinations possible, because, although involving all the resistances, and although the same for each current, it has not been formed in a symmetrical fashion. It has been obtained by considering  $(n+1)$  branches no three of which meet in a point.

In the expression for  $\Delta$  we must therefore exclude those combinations involving the product of the resistances of any three branches meeting in a point.

$\Delta$  consists of the sum of the products of the resistances of all combinations of  $n+1$  branches which are such that independent currents may be supposed to exist in them

...      ...      ...      ..      ...      ...      (7)

If we diminish to zero the resistance of any one branch of the network,  $\Delta$  will be diminished by the sum of the products containing this resistance as a factor. We cannot introduce new products by this means.

Now let  $a b c \dots x$  be any  $(n+1)$  branches of the network, and suppose we diminish the resistances of the remaining  $(2n-1)$  branches till they are infinitely small. It will still be true that

$$R_x = \frac{\Delta}{\text{coefficient of } x \text{ in } \Delta},$$

where  $x$  is any one of the  $(n+1)$  branches chosen.

Now  $\Delta$  will be infinitely small unless it contains as one of its terms the product  $a b c \dots x$ , since any other product of  $n+1$  resistances will contain at least one infinitely small

resistance as a factor. If  $\Delta$  does contain this product we shall have

$$R_{xx} = x + \text{terms infinitely small.}$$

Proceeding to the limit, we have

$$R_{xx} = x,$$

which means that the branch  $x$  has been short-circuited by making the  $(2n - 1)$  resistances zero.

$\Delta$  can therefore only consist of the sum of such products of  $(n + 1)$  resistances that the branches with which they correspond are each short-circuited by making the remaining  $(2n - 1)$  resistances zero. It must contain every such product, for if the remaining  $(2n - 1)$  resistances are zero we must have for each of the  $(n + 1)$  resistances

$$x = R_{xx} = \frac{\Delta}{\text{coefficient of } x \text{ in } \Delta},$$

which shows that  $\Delta$  must reduce to and contain the product  $a b c \dots x$ .

Now if the currents in the  $(n + 1)$  branches  $a b c \dots x$  are such that no one can be deduced from the others whatever the resistances of the remaining  $(2n - 1)$  branches, the  $(n + 1)$  branches in which they flow must be each short-circuited by making these resistances zero; otherwise three or more of the branches  $a b c \dots x$  will be brought together at a point which is only connected to the rest of the network by the branches so brought together. Under these circumstances one of the currents will be deducible from the remaining  $n$ , and this is contrary to supposition.

If, on the other hand, the currents in  $(n + 1)$  branches  $a b c \dots x$  are such that one is deducible from the rest whatever the resistances of the remaining  $(2n - 1)$  branches of the network, similar reasoning shows that these  $(n + 1)$  branches cannot be each short-circuited by diminishing the  $(2n - 1)$  resistances to zero.

The truth of (7) is now established.

Now, by definition,

$$R_{xy} = \frac{\Delta}{\Delta_{yx}},$$

and we may write  $\Delta_{yx} = \Delta_{xy}$ , since  $R_{xy} = R_{yx}$ .



Now  $\Delta$  is of degree  $(n + 1)$  and  $\Delta_y$  of degree  $n$ .  $\Delta_y$  cannot contain the square of any resistance; for suppose  $\Delta_y = z^2 \Delta_1 + \Delta_2$  where  $z$  is any branch which is neither  $x$  nor  $y$ . The only effect of making  $z$  infinite is to diminish the degree of the network by unity, for two points and three branches are removed. We shall get

$$R'_{xy} = \frac{\Delta'_y}{\Delta'_x},$$

where, since the degree of the network is now  $(n - 1)$ ,  $\Delta'$  will be of degree  $n$  and  $\Delta'_y$  of degree  $(n - 1)$ . If  $\Delta_y = z^2 \Delta_1 + \Delta_2$  and we make  $z$  infinite, the new value of  $R_y$  will be zero unless  $\Delta$  contains a term  $z^2 \Delta''$ . It is physically impossible for  $R_{xy}$  to be zero, for finite electro-motive forces will then produce infinite current, and if  $\Delta$  does contain a term  $z^2 \Delta''$  the new value of  $\Delta$  will be of degree  $(n - 1)$ , whereas it must be of degree  $n$ . Since  $x, y, z$  are any three branches of the network, it follows that

Neither  $\Delta_y$  nor  $\Delta$  can contain the square of any resistance in any of its product terms ... .. (8)

Now, since  $\Delta_y$  is symmetrical in  $x$  and  $y$ , it may be written

$$\Delta_y = (x + y) \Delta_1 + x y \Delta_2 + \Delta_3.$$

But, from physical considerations,  $R_y$  must become infinite whenever  $x$  or  $y$  becomes so.

$$\text{Now} \quad R_y = \frac{\Delta_y}{\Delta_x},$$

and  $\Delta$  contains no power of  $x$  or  $y$  higher than the first. It follows that  $\Delta_1$  and  $\Delta_2$  are both zero, and that

$$\Delta_y \text{ is independent of } x \text{ or } y \quad \dots \quad (9)$$

$\Delta_y$  must involve every resistance  $z$  which is neither  $x$  nor  $y$ , because otherwise  $\frac{\Delta_y}{\Delta_x}$  would become infinite when  $z$  is made infinite.  $\Delta_y$  must contain as a factor the expression which, when equated to zero, gives the condition that the branches  $x$  and  $y$  may be conjugate.

$$\text{Now} \quad \frac{R'_{xy}}{R'_{yz}} = \frac{\Delta'_{yz}}{\Delta'_{xy}},$$

and is independent of  $z$ , from (9).

$$\text{It also follows that} \quad \frac{R}{R_y} = \frac{\Delta}{\Delta_y},$$



resistance  $z$  and containing an electro-motive force  $E$ , and let  $E$  be the only electro-motive force in the network;

$$\text{then} \quad \frac{A - D}{E} = \frac{R_{(AD)}}{z + R_{(AD)}}.$$

But by Maxwell—Vol. I., sec. 281, equation (10)—we have in this case

$$\frac{A - D}{E} = \frac{2 \Delta'_{ad}}{z \Delta'},$$

where  $\Delta'$  is a determinant of the conductivities of the branches, and  $\Delta'_{ad}$  one of its minors. It follows

$$\frac{2 \Delta'_{ad}}{\Delta'} = \frac{z}{z + R_{(AD)}} \cdot R_{(AD)}.$$

$z$  in our case is infinite, so that

$$\frac{2 \Delta'_{ad}}{\Delta'} = R_{(AD)};$$

and Maxwell's equation of conjugacy (sec. 282A) becomes

$$R_{(AD)} + R_{(BC)} = R_{(AB)} + R_{(CD)}.$$

Now, whenever this is satisfied,  $\Delta_{xy}$  must vanish.  $\Delta_{xy}$  must therefore contain as a factor

$$R_{(AD)} + R_{(BC)} - R_{(AB)} - R_{(CD)} \quad \dots \quad (12)$$

Maxwell's investigation shows

$$\begin{aligned} R_{xy} &= \frac{2 x y}{R_{(AB)} + R_{(DC)} - R_{(AD)} - R_{(BC)}} \quad \dots \quad (13) \\ &= \frac{\Delta}{\Delta_{xy}} = \frac{2 x y}{R_{(AC, BD)}}, \text{ suppose.} \end{aligned}$$

Now, if  $x$  and  $y$  be any two branches, we may write

$$\Delta = x \Delta_1 + y \Delta_2 + x y \Delta_3 + \Delta_4 \quad \dots \quad (14)$$

where  $\Delta_1$ ,  $\Delta_2$ ,  $\Delta_3$ , and  $\Delta_4$  are independent of  $x$  and  $y$ ; whence

$$R_{(AC, BD)} = \frac{2 x y \Delta_3}{\Delta}, \text{ and } R'_{(AC, BD)} = \frac{2 \Delta_3}{\Delta_3},$$

where  $R'_{(AC, BD)}$  is the value of  $R_{(AC, BD)}$  when  $x$  and  $y$  are each infinite; or

$$R_{xy} = \frac{\Delta}{\Delta_{xy}} = \frac{2 \Delta}{\Delta_3 R'_{(AC, BD)}} \quad \dots \quad (15)$$

$\Delta_3$  being the coefficient of  $x y$  in  $\Delta$ .

$$\begin{aligned} \text{Now} \quad R_{xx} &= \frac{\Delta}{\text{coefficient of } x \text{ in } \Delta} \\ &= \frac{x \Delta_1 + y \Delta_2 + x y \Delta_3 + \Delta_4}{\Delta_1 + y \Delta_3}. \end{aligned}$$

change  $y$  to  $y'$ , and let the new value of  $R_{xx}$  be  $R'_{xx}$ : we easily obtain

$$\frac{1}{R'_{xx}} = \frac{1}{R_{xx}} + (y' - y) \frac{\Delta_1 \Delta_2 - \Delta_3 \Delta_4}{\Delta \Delta'},$$

where  $\Delta'$  is the value of  $\Delta$  when  $y'$  is put for  $y$ .

But from (19) we obtain

$$\frac{1}{R_{xx}} = \frac{1}{R'_{xx}} + R_{xy} \frac{y' - y}{R'_{xy}},$$

and from (9) we have

$$\Delta_{xy} = \frac{\Delta}{R_{xy}} = \frac{\Delta'}{R'_{xy}};$$

whence

$$\Delta_{xy}^2 = \Delta_1 \Delta_2 - \Delta_3 \Delta_4 \quad \dots \quad \dots \quad (16)$$

so that  $\Delta$  possesses the property that whatever branches  $x$  and  $y$  be chosen  $\Delta_1 \Delta_2 - \Delta_3 \Delta_4$  is a perfect square (see 14), and whenever two branches are conjugate  $\Delta$  breaks into factors.

$$R_{xy} = \frac{\Delta}{\pm \sqrt{\Delta_1 \Delta_2 - \Delta_3 \Delta_4}} \quad \dots \quad \dots \quad (17)$$

This determines  $R_{xy}$  in all but the sign. If it is only necessary to determine the integral of the current through one branch of the network, the network functions may be considered positive quantities if the positive direction of any branch be determined according to the definition already given. Otherwise a direction in each branch must be chosen arbitrarily as the positive direction, and the resistance function  $R_{xy}$  must be defined according to equation (13) or equation (15), where A and B are the positive ends and C and D the negative ends of the two branches respectively. In this case Maxwell's investigation shows that a positive electro-motive force  $E_x$  in the branch  $x$  will produce a current  $E_x/R_{xy}$  in the positive direction of the branch  $y$ . With this convention we can generalise equation (5); for if  $w$  be any branch of the network, and if  $x, y, z$  be any three branches meeting in a point, we have

$$\frac{1}{R_{xx}} + \frac{1}{R_{yy}} + \frac{1}{R_{zz}} = 0 \quad \dots \quad \dots \quad (18)$$

where the branches are supposed to have their positive ends at the point. If one of the branches has its negative end at the

point the sign of the corresponding network function must be changed. This equation but expresses the fact that the accumulation of electricity at the point is zero.

#### SOME FURTHER RELATIONS BETWEEN THE NETWORK FUNCTIONS.

In the simple case of the Wheatstone's bridge many relations follow from Lord Rayleigh's\* theorem that if the current in any arm  $s$  of the bridge be  $C_s$ , and if  $s + \sigma$  is the value of  $s$  for which the bridge will be balanced, the current through any branch of the bridge will be exactly as if the resistance  $s$  were increased to  $s + \sigma$ , provided we introduce into the new arm ( $s + \sigma$ ) an electro-motive force  $C_s \sigma$  acting in the direction of the current  $C_s$ .

Indeed, from the general fact that  $\frac{R_{xy}}{R_{xx}}$  is independent of  $x$ , it follows that

$$\frac{R_{ss}}{R'_{ss}} = \frac{R_{sp}}{R'_{sp}} = \frac{R_{sb}}{R'_{sb}} = \frac{R_{sq}}{R'_{sq}} = \frac{R_{sr}}{R'_{sr}},$$

where the quantities with a dash denote the corresponding quantities without a dash after we have substituted  $s + \sigma$  for  $s$  in them.

Also, each of the above expressions is equal to  $\frac{\Delta}{\Delta'}$ , since

$$\Delta' = \Delta + \sigma B,$$

where  $R_{ss} = \frac{\Delta}{B}$  and  $R'_{ss} = \frac{\Delta'}{B}$ .

Moreover, for the Wheatstone's bridge we know

$$R_{pb} = \frac{\Delta}{p\sigma} \quad R'_{pb} = \infty$$

$$\therefore \frac{p\sigma}{\Delta} = \frac{1}{R_{pb}} = \frac{\sigma}{R_{sb}} \cdot \frac{1}{R'_{sq}} = \frac{\sigma}{R_{sq}} \cdot \frac{1}{R'_{sb}}$$

by the above-mentioned theorem.

It follows that  $\Delta'$  breaks into factors—a fact easily verified, and one the statement of which is equivalent to Lord Rayleigh's theorem

$$\Delta' = p R'_{sq} R'_{sb}$$

or

$$p \Delta' = \Delta'_{sq} \Delta'_{sb}.$$

The same theorem gives—

$$\frac{1}{R'_{sq}} = \frac{1}{R'_{sq}} + \frac{\sigma}{R_{ss} R'_{sq}} = \frac{1}{R'_{sq}} + \frac{\sigma}{R_{sq}} \frac{1}{R'_{sb}},$$

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\* "Determination of B.A. Unit," *Phil. Trans.*, Part II., 1882, page 677.

$$\frac{1}{R_{ab}} = \frac{1}{R'_{ab}} + \frac{\sigma}{R_{aa}} \frac{1}{R'_{aa}} = \frac{1}{R'_{ab}} \left( 1 + \frac{\sigma}{R_{aa}} \right),$$

$$\frac{\Delta'}{\Delta} = 1 + \frac{\sigma}{R_{aa}} = \frac{1}{1 - \frac{\sigma}{R'_{aa}}},$$

$$\frac{1}{R_{bb}} = \frac{1}{R'_{bb}} + \frac{\sigma}{R_{ba}} \frac{1}{R'_{aa}};$$

and other relations can be written down.

The theorem stated above can be proved and generalised as follows:—

Let  $s$  be the resistance of any branch of any network of conductors; let  $E_s$  be the electro-motive force,  $C_s$  the current, and  $V_s$  the difference of potentials of the extremities of the branch  $s$ . Suppose that  $E_s$  and  $V_s$  both tend to maintain the current  $C_s$ . Then

$$s C_s = V_s + E_s.$$

If we have a second network which differs from the first only in the branch  $s$  we shall have

$$s' C'_s = V'_s + E'_s,$$

where, by supposition,  $V_s = V'_s$ ,  $C_s = C'_s$ . This can only be the case when

$$E'_s - E_s = (s' - s) C_s.$$

If this relation holds, and if the resistances and electro-motive forces of the second network differ only in the branch  $s$ , we shall have

$$V'_s - V_s = s' (C'_s - C_s).$$

If  $C'_s = C_s$ , the currents in corresponding parts of the two networks will be the same, for  $V_s$  and  $C_s$  being known the currents in the network outside  $s$  are perfectly determinate, and will be the same for the two cases if  $V_s$  and  $C_s$  are so.

Now suppose the change to take place in two parts.

First alter the electro-motive force  $E_s$ : the current changes according to the equation

$$C''_s - C_s = \frac{E''_s - E_s}{R''_{ss} - R_{ss}},$$

since the change is entirely due to the change of the electro-motive force  $E_s$ ; and

$$E''_s - E_s = R''_{ss} (C''_s - C_s),$$

since

$$R''_{ss} = R_{ss} \text{ by supposition.}$$

Now suppose  $s$  be changed to  $s'$ , so that  $R''_{xy}$  is changed to  $R'_{xy}$  where  $R'_{xy} - R''_{xy} = s' - s$ : we have

$$\begin{aligned} C'_x - C''_x &= \sum \frac{E_x}{R'_{xy}} - \sum \frac{E_x}{R''_{xy}} \\ &= \sum \frac{E_x}{R'_{xy}} \left(1 - \frac{R'_{xy}}{R''_{xy}}\right) = \sum \frac{E_x}{R'_{xy}} \left(1 - \frac{R'_{xy}}{R''_{xy}}\right) \end{aligned}$$

by (10); or

$$C'_x - C''_x = C'_x \left(1 - \frac{R'_{xy}}{R''_{xy}}\right);$$

that is,

$$R''_{xy} C''_x = R'_{xy} C'_x.$$

Now suppose that  $E''_x - E_x = (s' - s) C_x$ :

then

$$E''_x - E_x = (R'_{xy} - R''_{xy}) C_x;$$

but

$$E''_x - E_x = R''_{xy} (C''_x - C_x);$$

whence

$$\begin{aligned} R''_{xy} C''_x &= R'_{xy} C_x \\ &= R'_{xy} C'_x \end{aligned}$$

or

$$C'_x = C_x.$$

Thus, if  $C_x$  be the current in any branch of any network of conductors, and if the resistance  $s$  of the branch be increased to  $s'$ , the currents will be the same in every part of the network, provided an additional electro-motive force be inserted in the branch  $s$  aiding the current and equal in amount to

$$(s' - s) C_x.$$

This is expressed analytically by the equation

$$\frac{1}{R'_{xy}} = \frac{1}{R''_{xy}} + \frac{s' - s}{R'_{xy} R''_{xy}} \quad \dots \quad \dots \quad (19)$$

$x, y, s$  are any three branches of the network, any two or all three of which may be identical.

#### EXAMPLES.

Consider Maxwell's method of determining capacity (Vol. II, sec. 776). Suppose the commutator be placed in the arm  $s$  of the bridge. By equation (3) the formula is

$$\frac{E t}{R_{bg}} = 2 K V \left[ \frac{s}{R_{bg}} \right]_{s=\infty}$$

where  $V$  is the highest potential to which the condenser rises. Now

$$V = \left[ C_s \right]_{s=\infty} = \left[ \frac{s E}{R_{bg}} \right]_{s=\infty}$$



since this is the potential difference of the condenser when the commutator does not revolve. The derangement from balance is  $\sigma = s' - s$ , where  $s' p = q r$ ; whence

$$\frac{1}{R_{sq}} = \frac{\Delta'}{\Delta} \frac{1}{R'_{sq}} \quad \frac{1}{R_{sb}} = \frac{\Delta'}{\Delta} \frac{1}{R'_{sb}}.$$

Now 
$$\frac{\Delta'}{\Delta} = \frac{1}{1 - \frac{\sigma}{R'_{sb}}} = \left(1 + \frac{\sigma}{R'_{sb}}\right);$$

whence

$$\frac{t}{R_{sq}} = \frac{2K}{R'_{sq} R'_{sb}} \times \left[ \frac{s^2}{\left(1 - \frac{\sigma}{R'_{sb}}\right)^2} \right]_{s=0} = \frac{2K}{R'_{sq} R'_{sb}} (R'_{sb})^2.$$

Now 
$$\frac{1}{R_{sq}} = \frac{1}{R'_{sq}} \left[ \frac{s}{R'_{sb}} \right]_{s=0} = \frac{R'_{sb}}{R'_{sq} R'_{sb}}.$$

Substitute, and we get

$$K = \frac{t}{2 R'_{sb}} = \frac{t \text{ coefficient of } s' \text{ in } \Delta'}{2 \Delta'},$$

which is Maxwell's solution.

By writing out the value of  $R'_{sb}$  as above defined, and remembering that  $\Delta'$  breaks into factors, this formula will be found identical with that given by Professor J. J. Thomson.

Self-induction of the galvanometer has no effect whether the condenser gets fully charged or not. Professor Ayrton has noticed misprint of  $\alpha$  for  $a$  in Maxwell (Vol. II., sec. 776).

As another example, take Ayrton and Perry's method of determining self and mutual induction.

Integrating equation (2), during the time  $t$  that the galvanometer and battery are on together we have in this case

$$Q_1 = \frac{E t}{R_{sq}} - \frac{1}{R_{sq}} \left\{ \frac{E L_s}{R_{sb}} + \frac{E M_{sb}}{R_{bb}} \right\} - \frac{1}{R_{sq}} \left\{ \frac{E L_b}{R_{bb}} + \frac{E M_{sb}}{R_{bs}} \right\} - \frac{1}{R_{sq}} \frac{E L_g}{R_{bg}}.$$

While the galvanometer is short-circuited the additional quantity which rushes through is

$$Q_2 = + \frac{1}{g} \frac{E L_g}{R_{bg}},$$

since  $g$  is the new value of  $R_{gg}$ . Thus the quantity  $Q_1 + Q_2$ , passing through the galvanometer in each cycle is zero if

$$t + \frac{L_g}{g} - \frac{L_g}{R_{gg}} - \frac{L_b}{R_{bb}} - \frac{M_{sb}}{R_{bb}} = \frac{R_{bg}}{R_{sq} R_{bb}} L_s + \frac{R_{bg}}{R_{sq} R_{bb}} M_{sb}.$$

Now 
$$\frac{R_{b_j} R_{a_j}}{R_{a_j} R_{b_j}} = \frac{R_{b_a} R'_{a_j}}{R_{a_j} R_{b_a}} \frac{1}{\sigma} = \frac{R'_{a_j}}{\sigma R_{a_j}} = \frac{\Delta'}{\sigma \Delta} = 1 + \frac{\sigma}{R_{b_a}},$$

since 
$$\frac{1}{R_{b_j}} = \frac{\sigma}{R_{b_a} R'_{a_j}};$$

moreover, 
$$\begin{aligned} \frac{R_{b_j}}{R_{a_j} R_{b_j}} &= \frac{1}{\sigma} \frac{R_{b_a} R'_{a_j}}{R_{b_a} R_{a_j}} = \frac{\Delta'}{\Delta \sigma} \left\{ \frac{R_{b_a}}{R'_{b_a}} + \sigma \frac{R_{b_a}}{R_{b_a} R'_{b_a}} \right\} \\ &= \frac{1}{\sigma} \left\{ \frac{R'_{b_a}}{R'_{b_a}} + \frac{\sigma}{R_{b_a}} \right\} = \frac{1}{\sigma} \left\{ \frac{p+r}{p} + \frac{\sigma}{R_{b_a}} \right\} \end{aligned}$$

whence we obtain

$$L_s + \frac{p+r}{p} M_{ab} = \sigma t + \sigma \left\{ \frac{L_g}{g} - \frac{L_g}{R_{gg}} - \frac{L_b}{R_{bb}} - \frac{L_s}{R_{ss}} - \frac{2 M_{ab}}{R_{ab}} \right\}$$

This solution is identical with that worked out in Ayrton and Perry's paper.

For the method indicated by Fig. 17 we have, if X be joined to C,

$$Q_c = \frac{E t}{R_{bg}}, \quad -Q_p = -\frac{M_{rb} E}{R_{bb} R_{rg}} - \frac{L_g E}{R_{bg} R_{gg}} + \frac{L_g E}{g R_{bg}} - \frac{M_{rb} E}{R_{bg} R_{rb}},$$

where  $t$  is the time in a cycle that the galvanometer and battery are on together. We get for balance

$$\frac{s+q}{s} M_{rb} = - (p+r) t - (p+r) \left\{ \frac{L_g}{g} - \frac{L_g}{R_{gg}} - \frac{2 M_{rb}}{R_{rb}} \right\}.$$

For the method indicated in Fig. 18 we have, in the case when  $r$  is infinite,

$$Q_c + Q_d = \frac{E t_1}{R_{bg}} + K E \rho_1 \left( \frac{b_2}{R_{ba}} + \frac{g}{R_{bg}} \right),$$

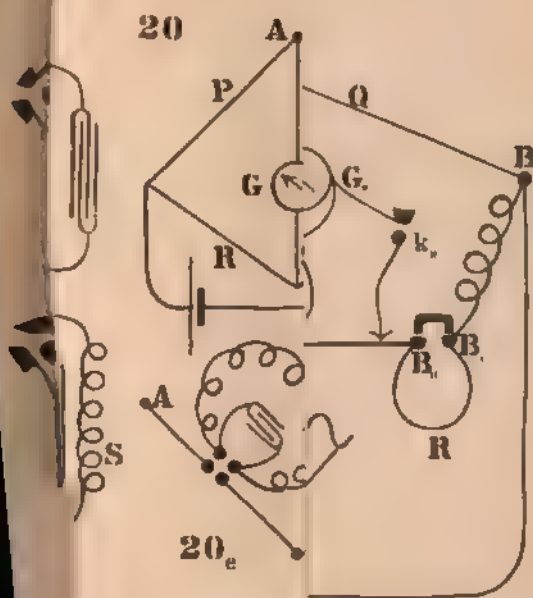
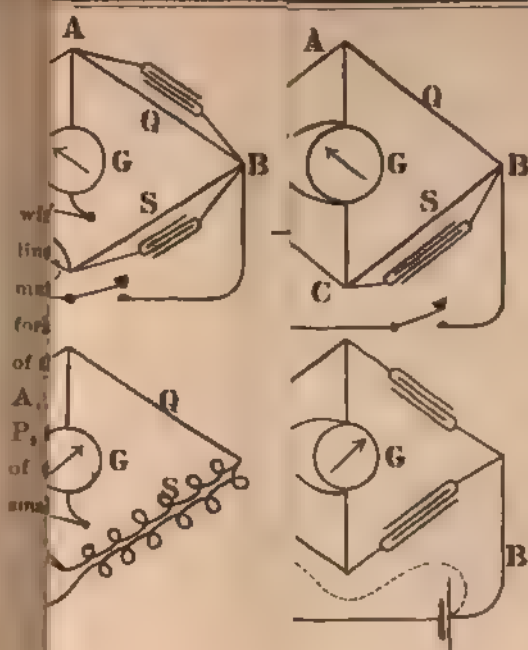
where  $t_1$  is the time of charging;

$$Q_c + Q_d = \frac{E t_2}{R_{bg}} - K E \rho_2 \left( \frac{b_2}{R_{bb}} + \frac{g}{R_{bg}} \right),$$

where  $t_2$  is the time of discharging. The whole quantity is zero if

$$t_1 + t_2 = K (\rho_2 - \rho_1) \left\{ b_2 \frac{s+q}{s} + g + g \right\},$$

where, if we imagine the condenser replaced by a battery, the proportion of the current through the galvanometer to that through the battery will be  $\rho_1$  when the free end of the battery is connected with the wire  $b_2$ , and  $\rho_2$  when it is connected with the galvanometer. Now, generally,  $s$  will be small compared with  $b_2$  or with  $g$ , so that  $\rho_1$  may be neglected compared with  $\rho_2$ , and



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$$\rho_s = \frac{s + q}{g + s + q}, \text{ practically;}$$

so that

$$K = \frac{s}{q} \frac{1}{b_s} t,$$

where  $t$  is the period of the commutator.

For the case of Fig. 9, when the resistance  $s$  is infinite, we have for balance the condition

$$\frac{E t}{R_{gg}} = K V \left[ \frac{s}{R_{ss}} \right]_{s=\infty}$$

neglecting the self-induction of the galvanometer,  $V$  being the potential difference to which the condenser rises, and, as before,

$$V = \left[ s C_s \right]_{s=\infty} = E \left[ \frac{s}{R_{ss}} \right]_{s=\infty}$$

and, as in Clerk Maxwell's method, we get

$$t = K R'_{ss};$$

in fact, the method is essentially the same as that of Maxwell. Its connection with Maxwell's method of determining self-induction (see Fig. 7) is clearly shown by deducing one formula from the other.

We have for self-induction—

$$L_s = \frac{q^2 r - p^2 s}{p} \left( t + \frac{L_g}{g} - \frac{L_{gg}}{R_{gg}} - \frac{L_s}{R_{ss}} \right);$$

we have for capacity—

$$-s^2 K_s = \frac{q^2 r - p^2 s}{p} \left( t + \frac{L_g}{g} - \frac{L_{gg}}{R_{gg}} + \frac{s^2 K_s}{R_{ss}} \right).$$

Now  $R_{ss} = s + R_{ss} = s + R_s$ , using Maxwell's notation;

also,

$$\frac{p^2 s - q^2 r}{p} = s - R_1 \quad \quad \quad \text{,,} \quad \quad \quad \text{,,}$$

whence  $\frac{s^2 K_s}{(s - R_1)(s + R_s)} (R_1 + R_s) = t + \frac{L_g}{g} - \frac{L_{gg}}{R_{gg}}.$

Now make  $s$  infinite, and we have

$$K_s (R_1 + R_s) = t + L_g \left( \frac{1}{g} - \frac{1}{R_{gg}} \right).$$

Professor D. K. HUGHES: I have taken a very great interest in Professors Ayrton and Perry's remarkable paper, in which they describe the method and instrument employed by them to resolve the problem of the commercial measurement of the coefficient of self-induction, and I congratulate those gentlemen upon the

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distinct advance they have made upon all previous methods for the object they have in view. They describe clearly that the instrument and method are not for the investigation of the phenomenon of self-induction, but for the measurement of its coefficient, and for this purpose their method is admirably suited.

The object of my researches upon self-induction, which I have published, was different, my object being to investigate the phenomenon itself. For this purpose I found that the method which I employed was extremely sensitive, as it allowed me to perceive the smallest changes in wires of but a few inches in length, and, by a system of balancing each effect separately, to analyse or separate the effects due to the extra currents from those due to a resistance caused by the electro-magnetic inertia.

We now know that the difference previously remarked between iron and copper wires for telegraphic purposes is entirely due to self-induction, and that where rapid currents are transmitted, as in high-speed telegraphic instruments and the telephone, the copper wire allows a transmission of at least four times greater speed, or to a greater distance than is possible with an iron wire of circular section.

Let us observe the cause of the apparent extra resistance shown by a coil or straight wires during the variable period. We know that there are two extra currents—the first on the passage of the primary current, in the opposite sense to this current, and the second on the cessation of the primary, in a similar direction to the primary. If we reduce these currents to zero by an opposing extra current of exactly similar force and duration of time, a sluggish instrument, such as a galvanometer, might lead us to suppose that we had then a perfect zero; but a more rapid instrument, such as the telephone, at once shows that we cannot perfectly balance or produce a zero by this method—there is still a large residual effect which can only be balanced by introducing an extra resistance on the opposite side of the bridge. We have thus not only the electro-motive force of the extra currents to balance, but a resistance which can only be balanced by an equivalent opposing resistance. The extra resistance of a wire

during the variable period can be demonstrated to be due to its electro-magnetic inertia. Professor  
Huxley.

Experiments show that the form as well as the nature of the conductor has an immense influence, for we can reduce the self-induction of a straight copper wire by changing the circular form of the wire into that of a thin flat ribbon, and iron, although possessing an extremely high magnetic permeability, can be reduced almost to the low specific inductive capacity of copper by the prevention of the complete formation of the circular magnetism which can be now demonstrated to exist only on the exterior surface of the iron wire; thus thin flat strips of iron, or stranded wires, have far less self-induction than that of one of solid circular section.

To prove that the high coefficient of self-induction of an iron wire is due entirely to the formation of circular magnetism on its exterior surface, I compared wires of iron with those of a compound formation, such as an iron wire coated with an exterior coating of copper, or reversing the arrangement and coating a copper wire with an external coating of iron, and taking as a standard of comparison a pure copper wire of similar length and diameter. I found that an iron wire had, with periodic primary currents of 23,000 per minute, four and a half times greater self-induction than copper, or, taking copper as 100, iron was 460; coating the iron wire with copper at once reduced the iron wire to 115, or but little higher than a solid copper wire—due entirely to the comparative absence of the strong circular magnetism which exists even in solid wire to a comparatively small depth on the exterior of the wire. Now, if this view is correct, we should have almost as strong an effect by merely coating a copper wire with iron as with a solid iron wire itself. This proves not only to be the case, but we have actually a higher degree of self-induction from a copper wire coated with iron than from a solid iron wire, or 550 for the copper coated with iron, as compared with 460 for the solid iron wire.

To investigate the cause of this curious increase of effect, and suspecting that the cause was due to a phenomenon which I had previously observed during my researches upon magnetism—



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namely, that magnetic inertia is reduced by the passage of an electric current through the iron—I insulated the exterior coating of iron from the interior copper wire, employing for this purpose an insulated copper wire in the interior of an iron tube (the copper wire was in connection with the bridge, whilst the exterior iron could be joined in the same circuit or insulated as desired); the effect being that when the exterior tube was insulated its reaction on the interior wire from its electromagnetic inertia was far greater than when the current was passing through it, the comparative value of the self-induction of the internal copper wire being 720 when the tube was insulated, against 61.5 when the current passed through the tube.

We can demonstrate by a different method not only the inertia possessed by the magnetic molecules of iron, but the influence of an electric current in diminishing the inertia or allowing greater freedom to the rotation of the magnetic molecules in any direction quite independent of the direction of the electric current. I have made numerous experiments on this subject, and I will cite one which can be easily repeated by the most simple means.

Let us take a neutral bar of soft iron of  $\frac{1}{4}$  inch, and from 20 to 30 inches in length. If we place this bar horizontally east and west, and observe by means of a magnetometer, there should be no longitudinal magnetism due to the earth's influence; but if we approach a small permanent magnet to the distant end of the iron, we find at once that the iron has a certain degree of magnetic conductivity. If we now strike the iron bar, or by any means give it mechanical vibrations, the mechanical effect is to loosen the molecules, and at once a far higher degree of magnetic conductivity becomes apparent; we find also the same effect on taking away the permanent magnet, for the bar then shows strong proofs of its retention of magnetism, which, however, disappear when the bar is struck. We find precisely similar results if, instead of mechanical vibrations being given to the iron, an electric current is passed through the bar, for it will then show a higher magnetic conductivity and a less retention than if the bar was untouched. That this effect is due to the loosening of

the molecules, and not due to any directive tendency of the current, may be proved by sending the current longitudinally through the bar in either direction, or passing the current transversally through the bar, drawing the connecting wires along the bar; the effect in all cases being precisely similar to the loosening effects given by mechanical vibrations. Thus we have less magnetic inertia when an electric current passes through an iron wire than we should have if the exterior coating or tube was insulated. I have tried to apply this effect to the production of electromagnets of a higher magnetic efficiency for rapid currents than otherwise possible by passing the current through the coil and iron core, or by employing a separate local battery of low resistance to pass only through the core. I have not, however, been able to obtain more than 25 to 40 per cent. increased efficiency of the magnet, and it would be of no practical value except for special applications.

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In mentioning these researches I wish to show that when we are investigating any phenomenon, such as self-induction, we should not confine ourselves to any particular method, choosing that by preference which will better demonstrate or bring into evidence the effects which other methods might neglect.

The object, however, of Professors Ayrton and Perry, as they have well explained, is entirely different: they desire to measure and express in known terms the coefficient of self-induction; and I must congratulate them on their great success, and assure them that I hold in the highest appreciation the masterly paper which they have presented to our Society.

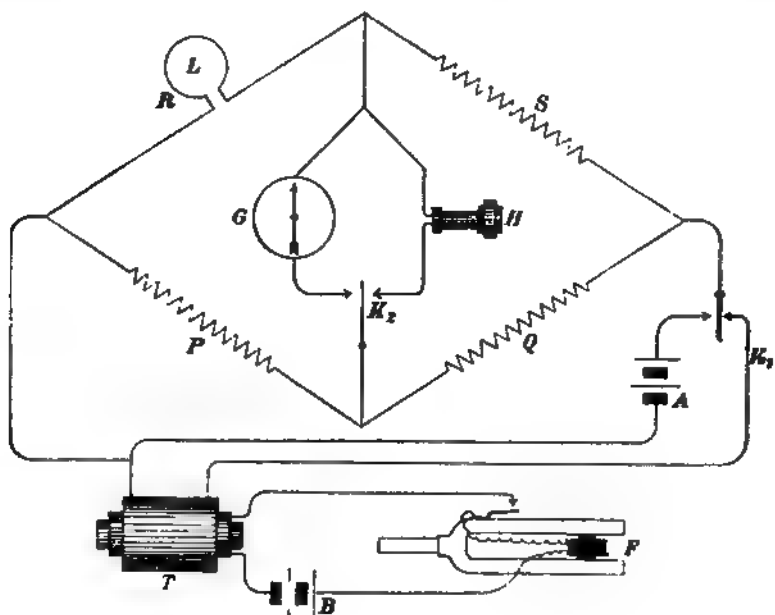
Professor S. P. THOMPSON: The Society has to congratulate itself on a really very important addition to our practical knowledge on the subject of self-induction. We have now really the means of acquainting ourselves with the actual working values of coefficients of self-induction; and it will be certainly our own fault if we do not acquire the same kind of working ideas as to how much self-induction exists in different pieces of apparatus as we now have about their particular number of ohms, or, in the case of batteries, their particular number of volts. We should think ourselves very ignorant if we had a dynamo and did not

Professor  
Thompson.

Professor  
Thomson

know how many volts it was capable of working at. I suppose that ten years hence we shall be equally *au fait* with the coefficients of self-induction of our apparatus. If I might pass one word of criticism, it is a very mild one; it is this: I should like to have seen some mention made of a method of measuring the coefficient of self-induction devised by M. Joubert, and which depends upon the measurement of the apparent resistance when an alternating current of known period, and of a reasonably simple harmonic kind of variation, is passed through the coil whose self-induction is desired to be measured. I have tried that method several times, one way or another, to see whether with ordinary laboratory apparatus one could get anything like a reasonable result. The only difficulty arises in obtaining an absolutely known period to one's alternating current; but that can be got over to a certain extent by employing as an interruptor a tuning-fork worked electro-magnetically. I have a tuning-fork that has for its period exactly 100 complete vibrations in a second; it is a very convenient number for calculating resistances of the usual kind, without self-induction. There is a battery (A), and a key (K), which can be used as an ordinary battery key, but which can also be used to change the circuit so as to switch in, in place of the battery, the secondary wire of a small induction coil (T), the primary of which contains the interruptor (F), the frequency of vibration of which is known, and a local battery (B). I found, however, that if one merely introduced the fork into the circuit, and attempted to measure the resistance both when the fork was interrupting the current and when it was not allowed to interrupt, one obtained contradictory and inconsistent results for different strengths of current. This, however, I found to be obviated by putting the interruptor in the primary circuit of an ordinary induction coil the "break" of which was screwed up hard, and the secondary coil used in the circuit in which one was going to measure the resistance. That is a simple modification of Joubert's method, and I believe it to be a thoroughly practical method, capable of being readily used in connection with the Wheatstone bridge. Its arrangement is as I have shown in the figure. Let one arm of the bridge be constituted by the wire the

coefficient of self-induction of which is wanted, and let us call its ordinary resistance  $R$ ; the other arms of the bridge ( $S$ ,  $P$ , and  $Q$ ) being other resistances.



In the galvanometer part of the bridge a two-way key ( $K_2$ ) allows the use either of the ordinary galvanometer or of some apparatus adapted for detecting alternating currents, such as a Bellati's electro-dynamometer, or, more simply, a telephone receiver. The bridge is first adjusted for ordinary steady currents, using battery  $B$  and galvanometer; the true ohmic resistance ( $R$ ) being thus measured. Then, using the induction coil and interruptor, and the telephone receiver, a new balance is obtained for the alternating currents, giving an apparent resistance ( $R'$ ) greater than the real resistance. In fact, we know that

$$R' = \sqrt{R^2 + 4\pi^2 n^2 L^2}$$

The fork which I have actually used as interruptor has exactly 100 vibrations per second; but it would be better to employ a fork having 159.1 vibrations. I choose this number because

Professor  
Thompson.

when multiplied by  $2\pi$  it becomes exactly 1,000, simplifying the calculation and giving

$$R'^2 = R^2 + (1,000 L)^2,$$

or

$$L = \sqrt{\frac{R'^2 - R^2}{1,000}},$$

which is an extremely convenient formula.

I would also express my great appreciation of the contribution made to this discussion by Mr. Sumpner. The series of diagrams that accompanies his paper, and the explanation which he has given of them, are a perfect revelation of the way in which the various methods of measuring these quantities dovetail into one another. He has generalised the Wheatstone bridge, and more than that, because he has let us see how one form is related to another. He has given us a sort of genealogical tree of Wheatstone bridges. I am particularly struck with Fig. 17; it seems to me a most remarkable thing that we should have all three methods of measuring mutual induction resolved into one in one single figure.

Professor  
Perry.

Professor J. PERRY: Perhaps, Sir, I might be allowed to say just a word about the method that Dr. Thompson has been speaking of. I forget Joubert's method, but I think I know what is meant. Really, the method depends on our having with accuracy a sine law of electro-motive force or potential difference at two points in the circuit, with constant resistance, and when this is the case the current is very easily expressed. Now I have gone very carefully into this question as to how it is possible to produce the simplest form of periodic variation of potential difference at two points in a circuit. You may do it by using a carefully formed alternating-current dynamo. But if you have a battery, how will you do it? Suppose you have a million cells of no resistance, you may shunt them out and in with great rapidity by moving a contact-maker according to a simple periodic law, perhaps; but can you do it by varying resistance? I know ways of varying resistance according to a simple periodic law. Well, I have gone into this, and I find that if you vary the resistance of your circuit according to the simplest periodic law, it does not vary the electro-motive force or current according to the

same periodic law; your current will vary by a rather complicated periodic law, and the complication becomes very great if you merely make and break the circuit. Now a bit of the method Dr. Thompson has explained comes in very usefully to eliminate the complications of the periodic law. It is known that self-induction, mutual induction, or any of these things, will flatten down the ripples—will eliminate complications——

Professor  
Perry.

Professor S. P. THOMPSON: Harmonic fly-wheel.

Professor J. PERRY: Will flatten down the ripples, and will tend to make a current which is a simple periodic function of time of the most complicated periodic function of the time; and though perhaps Dr. Thompson may not have thought the problem out, he was using a large amount of mutual induction to create such a simple periodic function of the time as would enable the elementary, the well-known simple law for alternating currents to be used in his calculations.

Mr. ARTHUR WRIGHT: I should like to ask Professor Ayrton Mr Wright whether the method of measuring the current from an alternating dynamo through a coil containing a very high coefficient of self-induction is not the simplest means of getting the coefficient of self-induction, if the alternations per minute of that current are known. The method that I should propose would be to keep a constant electro-motive force between the two terminals of the coil, and measure what current would pass through it. The self-induction of the coil can be found by a simple formula. This method, I know, in case of measuring the efficiency of transformers, forms a very ready means of determining their relative values: the smaller the current going through the coil, the higher the efficiency of the transformer.

I should also like to ask Professor Ayrton how he applies the secohmmeter when alternating currents are used. This is important, considering that nearly all currents in which self-induction effects of any magnitude occur are alternating. For instance, the telephone current is an alternating current; the rapid Wheatstone automatic transmitter telegraphic current, I believe, is also an alternating current; and the same with transformers. The results obtained with an intermittent direct

Mr. Wright. and an alternating current would greatly differ in measuring the induction of a coil with a closed magnetic current. As in practice nearly all instruments of any use have complete, or nearly so, magnetic circuits, and where self-induction effects rise to any importance the current is alternating, it seems nearly useless to measure the self-induction of a coil with an open magnetic circuit and an intermittent direct current.

The President.

The PRESIDENT: I have no doubt that Professor Ayrton is ready to reply to the questions that have been raised. I am extremely pleased to see that members other than those around this table join in the discussion upon these subjects, or ask questions. In my inaugural address I mentioned that I should like the general members of the Society to take more advantage of the opportunities for discussion than they have done hitherto. I will now call upon Professor Ayrton to reply.

Professor Ayrton.

Professor W. E. AYRTON, in reply, said: The modification of M. Joubert's method, that Dr. Thompson has so ably brought before us this evening, contains a very distinct addition and improvement if the lag due to self-induction be not found to introduce complications in the formula. The method still requires, however, the use of a delicate electro-dynamometer. Now, if you only possess a delicate electro-dynamometer, I can tell you of many modes of measuring self-induction; it is because people do not possess delicate electro-dynamometers as a rule that it seemed necessary to devise methods that do not require them. For instance, if you possess an electro-dynamometer, all you have got to do is first to obtain balance for steady currents then to make and break the battery circuit—do not do it so fast that the current cannot reach its steady value in the bridge with the battery circuit closed, and cannot die away when the battery circuit is broken—and the deflection of the dynamometer gives you the coefficient of self-induction without having any induction coil at all for the purpose of producing alternating currents. Take a tuning-fork, revolving commutator, or any mechanism which will make and break the current at a known rate, and at a rate which is not too large, then find the value of the deflection of the dynamometer by a method similar to that employed by us



with our less sensitive 1886 test for self-induction, and without our mechanism at all no doubt you can get the coefficient of self-induction. That is practically the very method that Dr. Fleming brought before us last time. The galvanometer that he brought before us was really an electro-dynamometer—the galvanometer with the suspended iron disc that, he said himself, acted for alternating currents by induced currents in the disc; and what was peculiar about that galvanometer, and, indeed, what makes it, I am afraid, rather hopeless for practical work, is that its whole action arises from the fact that there is a certain self-induction in the disc; it is small, but there is a very low resistance, so that there is a certain measurable time constant of the suspended disc, and hence it is that an effect is obtained.

Now I would answer Professor S. P. Thompson by saying that if I had a very delicate electro-dynamometer then I would simply make and break the battery circuit; and I would answer Dr. Fleming by saying that the use of such a dynamometer would be preferable to the use of the soft iron disc galvanometer, because an electro-dynamometer has well-known laws, whereas with the disc I am afraid it would be difficult without much experimenting to obtain results that are useful for determining a coefficient of self-induction in seconds.

With reference to Mr. A. Wright's question, of course if you are not dealing with a closed magnetic circuit, but with an ordinary electro-magnet which was not closed, say a straight solenoid with a piece of iron in it, the coefficient of self-induction for ordinary currents measured by the secohmmeter is the coefficient of self-induction which you would obtain when you are using alternating currents; but Mr. Wright is perfectly correct in his suggestion that if you want to measure the coefficient of self-induction of a closed magnetic circuit you must no doubt alternate the current. Some of the students at the Central Institution have been measuring coefficients of self-induction of closed iron rings wound round continuously with wire, which could not be measured by making and breaking, because the magnetism would not die away if the current were merely interrupted. To make this measurement requires that

reverser  
yrion.

the -ecolmmeter should be different in this one respect—that instead of breaking the battery circuit it alternates the current: the current is alternated one way with the galvanometer operative, the galvanometer is then short-circuited and the current alternated back again, so that the second alternation produces no effect on the galvanometer; in fact, the whole difference is that, instead of breaking the battery circuit, we alternate it. It means, therefore, that the one commutator, which is a make-and-break in the apparatus before you, is replaced by a reversing commutator; the second commutator remains exactly as it is, and the operation is obviously as I have explained: the galvanometer is operative for the first reversal, the galvanometer is short-circuited when the second reversal takes place.

MR. ARTHUR WRIGHT: Is there any difference in the formula?

Professor AYRTON: Any material difference? No. There will be the difference, of course, that you have double your effect; that is all. You must halve what you get if you apply the formula we have given for merely making and breaking the battery circuit; there is no other difference.

In reply to Mr. Wright's other question, the method he describes can be used to measure a coefficient of self-induction, provided that the *exact* law of variation of the E.M.F. of the dynamo with the time be known. In the Ferranti machine it is a simple sine function, but with other dynamos it is probably a more complicated function, and this would tend to destroy the simplicity of the method that Mr. Wright suggests.

Professor Hughes has given us an extremely interesting account of his work that charmed the whole world rather more than a year ago. He has himself, I think, answered any criticism that he has made on the paper in saying that our object was a different one; indeed, the first paragraph of our paper would be an answer to any criticism if he had made it, viz., that the paper does not resemble his own in bringing out any new facts, for its aim is different—it is to help the practical electrician to obtain as clear an idea of self-induction as he has of resistance, so that the object was quite different. But Professor Hughes

would say, rightly, that the sechommeter does not measure all that can be measured. That is true. If you refer to Clerk Maxwell, you will find in the second edition, page 297, the following formula with an infinite series:—

$$E = RC + l(A + \frac{1}{2}) \frac{dC}{dt} - \frac{l^2}{1 \cdot 2} R \frac{d^2 C}{dt^2} + \frac{l^3}{1 \cdot 2 \cdot 3} R^2 \frac{d^3 C}{dt^3} - \frac{l^4}{1 \cdot 2 \cdot 3 \cdot 4} R^3 \frac{d^4 C}{dt^4} + \&c.$$

Now what did Professor Hughes do? what did we do? Professor Hughes really did this: he said, "People have gone "as far as the term  $l(A + \frac{1}{2}) \frac{dC}{dt}$ , and have neglected all the "terms that follow. I will investigate experimentally all the "remaining terms and see whether they have any practical value." I do not mean that Professor Hughes actually used these words or reasoned about this particular equation, but his reasoning, put into mathematical language, would take the form I have mentioned. He found that the neglected terms in this equation had a most important value in certain cases; he has told you the results that he obtained, and his results are expressed mathematically by the neglected terms in that equation. As Lord Rayleigh has mentioned, it was thought that the terms after the second were not of much consequence, and that the first two terms represented practically the whole effect; but Professor Hughes has shown by delicate measurement that, so far from these terms being negligible, they were not, and had to be taken into account. That was a great advance to our knowledge at the time, and I said so in this room as strongly as I could. But our object was to find  $l(A + \frac{1}{2})$ , or  $L$ , as it is shortly called; we wanted to devise a method for measuring  $L$  easily and accurately in absolute units, not to find out whether the other terms were negligible or not; and this is our answer to any criticism as to whether we have investigated the whole phenomena. No, we have not; we have not attempted to; but, knowing that the equation  $E = RC + L \frac{dC}{dt}$  was sufficient for ordinary work, we set ourselves to devise an easy practical way of finding  $L$  absolutely.

In reply to Professor Hughes's question as to whether the

Professor  
Ayrton.

Professor  
Ayrton.

secohmmeter gives the shape of the curve for the growth of a current in any particular circuit, I would answer that the secohmmeter is used to measure the coefficient of self-induction of any circuit; but, as is known, the curve of the growth of the current can be determined from the equation

$$C = \frac{E}{R} \left( 1 - e^{-\frac{R}{L}t} \right),$$

as soon as we know  $L$  and  $R$ , the coefficient of self-induction and the resistance of the circuit. Hence indirectly the secohmmeter helps us to ascertain the curve Professor Hughes has referred to.

It is impossible to express my admiration too strongly of the most masterly contribution that Mr. Sumpner has added to the original paper. When I read our own paper at the last meeting you may remember that I said, when I came to a reference to his name, that somehow or other I did not think the paragraph expressed all I felt; well, now you know it could not have expressed all I felt, because no one sentence could convey a satisfactory impression of all the work that he has done on this subject.

The additions that he has made are of great importance. I will not detain you three minutes in summing them up. He told you—what the world knew, no doubt, because they have used it—that a condenser shunted by a resistance acted like a negative self-induction. Mr. Preece has pointed out, as Mr. Sumpner said, that a condenser shunted by a resistance was actually used in telegraph circuits to balance the self-induction of the electromagnets of the receiver; but then Mr Sumpner naturally said, "If that is the case, then this apparatus, the secohmmeter, supplies a means of measuring capacity absolutely." Well, we have placed one of the latest forms of secohmmeter on the table, joined up for the absolute measurement of the capacity of this condenser in farads. Instead of having a ballistic galvanometer the periodic time of which has to be known as well as the logarithmic decrement, we have a simple Post Office galvanometer with only a pivotted needle and a simple Post Office bridge, one resistance coil which shunts the condenser, and a secohmmeter; and with that arrangement we can get the capacity of this condenser

absolutely. This very simple bit of apparatus gives the capacity absolutely in farads, and gives it by a very simple formula. Professor  
Ayrton.

No doubt there is the method suggested by Clerk Maxwell for determining the capacity of a condenser absolutely. It was employed by Mr. Glazebrook. The formula was worked out by Dr. Fleming in his very comprehensive paper on "Networks," and it has been discussed at some length by Professor J. J. Thomson, of Cambridge, in consequence, I venture to think, of a mistaken idea on his part that the reasoning employed by Maxwell was not rigid. With that method you may conveniently use a vibrating tuning-fork, and, as explained by Mr. Sumpner, there is no resistance shunting the condenser: you simply have the condenser in one arm of the bridge, and by some simple mechanism you charge the condenser while it forms one arm of the bridge, and you discharge the condenser not through the bridge; or you do *vice versa*: you charge not through the bridge, and you discharge it through the bridge; or, again, you simply alternate the connections of the condenser with the bridge, as originally suggested by Clerk Maxwell. The method is now well known, and the condenser connected in that way appears to act as a simple resistance, and can be replaced by a resistance. If any of you refer to the formula you will find it a very long one, and—I may just say this one word—I think it possible that it was a misprint in that formula that led Professor J. J. Thomson to conclude that Maxwell's reasoning was only an approximation; and in a paper communicated by Professor J. J. Thomson to the *Philosophical Transactions of the Royal Society* about 1883, he commences his examination of this very same method by stating that *Maxwell's reasoning was only approximate*. He then enters, apparently on that account, into a long train of reasoning, starting from the dissipation function, and obtains a formula which no doubt is not identical with Clerk Maxwell's on account of a printer's error, but which is identical with Maxwell's when the printer's error in Maxwell's is corrected. Professor J. J. Thomson does not say why he regards Maxwell's formula as being only an approximation, or why he considers that

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a mode of reasoning so much longer than that used by Maxwell has to be resorted to. Whether the misprint that I have referred to is at the bottom of it I know not. But, be that as it may, the secohmmeter method has at any rate one great charm: it gives an extremely simple formula which can be remembered. Mr. Sumpner has told you that when the secohmmeter is worked with the arrangement on the table there is an apparent diminution of resistance. Well, the apparent diminution of resistance, divided by the product of reading on the secohmmeter, multiplied by the square of the resistance shunted by the condenser, gives the capacity in farads; it is the apparent diminution in ohms divided by the product of the reading on the secohmmeter into the square of the resistance shunted in the condenser. That is simple enough, I am sure.

Then the next point that Mr. Sumpner brought out was an improvement on all Clerk Maxwell's methods of comparing or measuring self-induction, mutual induction, and so on; and he pointed out that it was a mistake altogether of Maxwell's attempting to get the zero method. Maxwell no doubt was imbued with the idea that a zero method must be better than any other, therefore he said, "Let us develop zero methods;" but when you try to make Maxwell's tests, it takes a very long time to make them, in consequence of the double adjustment necessary to obtain balance for a steady and for a growing current. Further, as Mr. Sumpner has pointed out in the particular case, which was not a selected case, the zero method was the least sensitive of all the arrangements that he adopted, so that nothing was gained in sensibility. The only thing gained by using Maxwell's method was that it formed a pleasant way of spending an afternoon. Then the next point of importance that has been developed since a fortnight ago is that it is not necessary to measure what may be called, for a moment, the static resistance of the coil *accurately* at all, for if you want to measure the coefficient of self-induction of the secohmmeter all you need do is to get a very rough approximation to the resistance; the spot of light is somewhere on the scale, and you do not worry yourself about the zero as long as the light is on the scale. Then, to get a balance when the secohm-



meter is rotating, you do not work either to the true zero or the zero that was used when working with the Wheatstone bridge, but to a mean position which depends upon the construction of the commutator. Mr. Sumpner did not mention that it is not absolutely necessary to even approximately measure the resistance of a coil before using the secohmmeter, since, as proved in an addition to our paper, the coefficient of self-induction can be accurately determined from two observations made at two different speeds with the secohmmeter without one's having any idea of the true resistance of the coil the coefficient of self-induction of which we wish to determine.

The next addition that Mr. Sumpner has made is that the secohmmeter can be used for all zero methods where we have to deal with intermittent currents; that is, all zero methods that have been devised, however perfect they may be, for measuring or comparing capacity, or self-induction, or mutual induction, can be made infinitely more sensitive by merely using this apparatus. The analogy that he gave you was a pretty one; it was this—that in ordinary measurements of a resistance you use, of course, steady currents, and the effect is produced by the electricity flowing for a long time. In the ordinary zero methods for measuring any of these three things—capacity, self-induction, and mutual induction—you can only get an instantaneous effect, because the effect is only produced on putting down or taking up the key. In fact, all the methods published for measuring self-induction, mutual induction, capacity, and so on, are analogous with the first rough approximation we make in measuring resistance: when we measure resistance by a bridge and have no idea of the value of the resistance we just tap the key—we know that it is unsensitive, and do not want it to be otherwise; and the ordinary measurements of self-induction are as unsensitive. Now this apparatus makes any of these zero methods as sensitive for the measurements of self-induction, mutual induction, or capacity as the ordinary steady currents are for the measurement of resistance.

I should like to have gone into the detail of the last experiment Mr. Sumpner referred to, but I will not do so at this late



Professor  
Ayrton.

hour, and to have spoken of a modified instrument, not for the direct measurement of self-induction at all, but to be used in connection with the method that Mr. Sumpner has described. The instrument is a modification of what we described some years ago as our "set-up voltmeter"—a spring voltmeter with a set to it. An ammeter with that device enables you to measure accurately the ratio which is required in the method, viz., the ratio of small change of current to the total current. There may be, for example, 20 or 30 ampères going through your dynamo in one case, or in another it may be a very small current; but in each case it is necessary to accurately measure a small change of current in order to ascertain the coefficient of self-induction. Well, this is done thus:—When the ammeter needle is at zero it may mean 5 ampères, 10 ampères, or 15 ampères; the spring itself is a very weak spring, so that the needle deflects right across the scale for the addition perhaps of half an ampère; you turn a milled head round to the mark which means 10 or 15, as the case may be, and you know then that when the needle begins to start it means, not a current nought, but 10 or 15 ampères going through the ammeter; and in each case the addition of a quarter of an ampère will deflect the pointer right across the scale. In fact, such an instrument is a combination of a zero instrument, like a Siemens' or Crompton's dynamometer, and a direct-reading instrument, like our ordinary ammeters. The result of the measurements Mr. Sumpner made was extremely pretty; I do not know whether the members all grasped it: it was that when a curve was drawn having for its abscissæ the various currents that were sent round the field magnets of the Ferranti dynamo, and for its ordinates the corresponding values of the self-induction obtained experimentally, there was observed to be a close agreement between the values of the ordinates and the differential coefficient of the magnetisation with respect to the current. The ordinates of the self-induction curve were small for small values of the current corresponding with the flat part of the curve of magnetisation, then the self-induction curve rose to a maximum corresponding with the steep part of the magnetisation curve, and, finally, it

approached the axis of current corresponding with the flat part of the magnetisation curve when the field magnets were saturated. Professor Ayrton.

There is one point that perhaps I may allude to. Since our last meeting I have been attacked, mildly it is true, for saying two things; and I am very glad that I have had the two attacks, because they balance one another. One attack that I have had made upon me was because I have defined the standard commercial current, the standard ampère, as being the current which precipitates 1.11815 milligrammes of silver per second in a certain standard silver nitrate solution; it has been said that that is not accurate enough. Who was it obtained by? It is the mean of the results obtained by Lord Rayleigh and F. and H. Kohlrausch for the electro-chemical equivalent of silver, and the mean differs but little from the result obtained by either. The objector says that the practical standard of current is the attraction of coils. Now I say it is not. The reason why I mention this is because I am coming to objection number two—about the unit of self-induction—in a moment. I say that the practical standard of current is not the attraction of one coil upon another; and it would be most difficult to determine whether an instrument was graduated correctly in ampères or not by comparing its readings with the attraction of two coils of known dimensions, unless you were a Lord Rayleigh, with the resources of a Cavendish laboratory. If you be a practical man, you will put your instrument in series with a silver voltameter and use the electro-chemical equivalent of silver that I have referred to for the purpose of deciding on the accuracy or inaccuracy of some particular current meter. For, as Kohlrausch has pointed out, a tangent galvanometer can be far more accurately calibrated absolutely by comparing its readings with the amount of chemical action produced in a given time in a voltameter than by trying to accurately determine the dimensions of the galvanometer and the absolute strength of the controlling magnetic field.

For the ordinary testing of ammeters you may use an electro-dynamometer as your standard instrument, because, having no iron, there is no residual magnetism; but with that electro-

Professor  
Ayrton.

dynamometer you do not attempt to calculate the attraction of one coil on another and so determine the absolute value of the current; what you do is to put it in series with a silver voltmeter and find the absolute values of the readings of the electro-dynamometer by direct comparison.

Therefore I believe that we may correctly define the current that deposits 1.11815 milligrammes of silver in a silver nitrate solution containing from 15 to 30 per cent of the salt as the practical ampère. The objection, however, raised to that definition was that it was not accurate enough. Well, perhaps the number is wrong by 1 in 10,000.

Take the other side of the question—that is, that we do not like to use the word “quadrant” for the unit of self-induction. Why? Because I say that an earth’s quadrant through Paris is not the commercial unit of self-induction. In this case it is said that I am too particular; but is that the case? The *mètre*, as you know, is intended to be one ten-millionth part of the earth’s quadrant through Paris: it is not quite so, and if it be so defined there is a distinct error made in principle, but only a very small one in actual practice; so that for practical purposes it does very much matter whether you say that it is the one ten-millionth part of the earth’s quadrant through Paris, or whether you say it is the length of a certain platinum iridium bar—the *mètre* prototype. Nevertheless, if any dispute ever arose about the accuracy of the length of any special *mètre* measure, it would be the *metre* prototype that would be referred to, not the earth’s quadrant.

But now with reference to the ohm the case is different. The difference between the true ohm and the legal ohm is 2.3 in a thousand, which can be easily detected with an ordinary Wheatstone’s bridge. To say, therefore, that a true ohm was a legal ohm, would not only be an error in principle, but would lead to a serious error in practice; and if you ordered a box of legal ohms, and the maker, with a mistaken idea about accuracy, sent you a box of true ohms, made with great accuracy, you would have a right to reject them as not being what you had ordered. We know that the legal ohm is not  $10^9$  centimètres per second, or practically one earth’s quadrant per second, which is the true

ohm. Therefore the earth's quadrant is not strictly the commercial unit of self-induction. The commercial unit is  $99,777 \times 10^4$ , and not  $10^9$  centimètres per second. There is a difference between the two far greater than any possible difference that can be found between the true ampère and Lord Rayleigh's ampère, I am certain; and therefore it seems absurd that on the one hand people should object to one's defining the commercial ampère as Lord Rayleigh's ampère, and on the other hand that they should say one is too nice when one makes a difference between the earth's quadrant and the commercial unit of self-induction, because that is exactly the same error as would be made if you were to confuse the legal ohm and the true ohm. Therefore my colleague and I do not take the earth's quadrant through Paris as the commercial standard of self-induction. The  $99,777 \times 10^4$  centimètres is, fortunately or unfortunately, the commercial unit. There is no doubt, I think, about that; what you like to call it is a totally different question. Whether you like to adopt our suggestion and call the unit a "secohm," is a different question; but you cannot say that the commercial unit is the true unit, because if you do you will confuse the legal ohm with the true ohm.

The PRESIDENT: Professor Forbes has kindly placed on the table for inspection some specimens of electric welding—Professor Elihu Thomson's process. You will all have read of the application of strong currents of electricity for welding purposes, and will no doubt take an interest in an examination of these specimens.

A ballot took place, at which the following were elected:—

*Member:*

Alexander St. Clair Taylor.

*Associates:*

John Brown, jun.

William Fowler.

Alfred Hay.

Howard Swan.

*Students:*

John Edward Mellor.

Alfred Sykes.

The meeting then adjourned til. May 26th, 1887.

Professor  
Ayrton.

The  
President.

The One Hundred and Sixty-eighth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, May 26th, 1887—Dr. JOHN HOPKINSON, F.R.S., Vice-President, in the chair.

The Chairman stated that in consequence of a slight accident to the President, Sir Charles T. Bright, he had been requested to occupy the chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced, and this being the last meeting before the recess, it was agreed, upon the motion of the Chairman, that, following the precedent of former years, the candidates should be balloted for that evening.

The following transfer was announced as having been approved by the Council, viz.:—

From the class of students to that of Associates —  
T. G. Ladds.

Donations to the Library of the Society were announced as having been received since the last meeting from Professor W. E. Ayrton, F.R.S., V.P., and from Messrs. Macmillan & Co., to whom the thanks of the meeting were unanimously voted.

The SECRETARY then read the following paper:—

## UNDERGROUND TELEGRAPHS.

By CHARLES THOMAS FLEETWOOD, Member.

In 1875 I had the honour of submitting to this Society a paper on the underground system of telegraph wires in the streets of London. The interesting discussion that followed the reading of that paper, showing the importance attached to the subject at that period, and the fact that at the present time many minds are earnestly engaged upon the subject of the best method of constructing lines of underground telegraph and telephone wires,

consequent upon the wholesale destruction of the overhead wires by the snowstorm of last December, have led me to think that a fitting opportunity has arrived for again offering a few facts referring to the history of subterranean wires, especially in the metropolitan district.

In the inaugural address of our President (Sir Charles Bright) we were reminded that this year (1887) is not only the Jubilee of our Most Gracious Sovereign Lady (Queen Victoria's reign, but also that of the year in which the great benefit of practical telegraphy was conferred upon this country and the entire world. While this is strictly true, I find it will be necessary to go back twenty-one years prior to that date—that is, to the year 1816—if we are to give credit to whom it is due in connection with buried conductors for the transmission of electricity.

It was in that year that our late benefactor Mr. (afterwards Sir Francis) Ronalds laid his first experimental line underground in his garden at Hammersmith, and convinced himself at least of the practicability of establishing telegraphic communication by means of insulated wires placed beneath the earth's surface. A small book published by him in 1823, entitled, "Description of an "Electric Telegraph and of some other Electrical Apparatus," gives a full account of the experiments referred to. A trough of wood about two inches square, well lined inside and out with pitch, was placed in a trench 525 feet long and 4 feet deep; lengths of stout glass tube were joined together by means of shorter lengths of similar tube of somewhat greater diameter, the joints being made with soft wax to allow for expansion and contraction caused by the variation of temperature. A copper wire was drawn through the glass tube, and the tube was embedded in pitch, the covering being screwed down while the pitch was hot. Here we have the germ of all future underground systems of telegraph, telephone, or electric light wires.

It has been said that Sir Francis Ronalds lived thirty years before his time; I am inclined to think he was fully fifty years before his contemporaries, for he suggested nearly everything that we do at the present time as regards underground lines—so much so, that it might be thought we are engaged in carrying out his plans.

He suggested that wires should be buried in iron troughs in trenches six feet deep in the middle of the high roads, and if there was any fear of the communication being interfered with by any mischievous person or persons, two different routes should be chosen. Testing stations should be established along the route, and linemen stationed at these points ready to start out after faults at any moment, should there be a necessity for so doing. He recommended that offices should be opened all over the country, and evidently foresaw the immense advantages that would be derived by the Government as well as by the commercial world in the employment of what he considered so "diligent a courier" as electricity. He richly deserved the honour that was conferred upon him by Her Majesty the Queen not long before his death at the age of 85 years. By the kindness of one of our Past-Presidents, Mr. Latimer Clark, a portion of the line laid at Hammersmith in 1816 is now upon the table (Fig. 1).

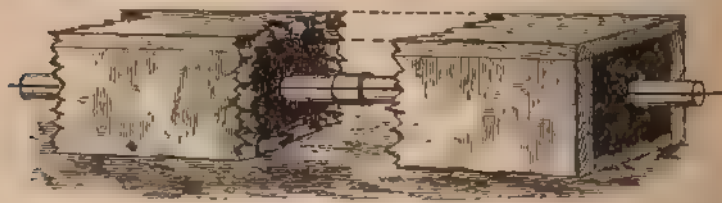


FIG. 1.

Messrs. Cooke and Wheatstone took out their first patent in 1837 for "Improvements in giving signals and sounding alarms in distant places by means of electric currents transmitted through metallic circuits." In this patent they included a plan for laying down subterranean wires, and in the same year established communication between Euston Square Station and Camden Town by means of five copper wires let into five grooves cut longitudinally in a piece of timber, the wires being covered with cotton and passed through a preparation of resin, and after being placed in the grooves, tongues of wood were placed over them to make all secure, and the whole was then covered with pitch and buried in the earth up what is known by the railway



men as "the incline" between the above-named points. A specimen (Fig. 2) of this line is here on the table, and can be



FIG. 2.

examined. Surprise may be expressed that the inventors should have expected it to maintain its insulation, yet have we not seen attempts being made during the last few years to construct electric light lines upon the same principle?

The second patent taken out by these pioneers included placing wires made up into cables in iron piping; this was in 1838. During the following year a line of five wires in an iron tube was put down on the Great Western Railway from Paddington to West Drayton, and afterwards on to Slough.

Conductors covered with cotton and passed through different solutions were tried in a variety of ways, some being suspended, and others buried in pipes, but very great difficulties were experienced in preserving the dielectrics; weak places being traced would be repaired with india-rubber tape and a solution of the same material. This kind of work was being done near the Stepney Station on the Blackwall Railway when Mr. Hatcher—then engineer to the Electric Telegraph Company—introduced to the lineman a piece of lead tubing containing several wires covered as above (Fig. 3).



FIG. 3.

The first line of wires erected upon poles for commercial

purpose was from Nine Elms to Grosport: this was before the London and South Western Railway was extended from Nine Elms to Waterloo Station. The Electric Telegraph Company opened their first office at 345, Strand, and decided to lay their wires underground from that point to Nine Elms Station. The route selected was over Waterloo Bridge, down Waterloo Road, through Oakley Street, Kennington Road, Kennington Lane, and thus to the railway. Two lead tubes about half an inch in diameter, covered with tarred yarn, each tube containing four copper wires wrapped with two layers of thick cotton, and the tube filled with a mixture of tar, resin, and grease, were drawn into a 3-inch cast-iron socket pipe. The lead tubes were originally fifty yard- in length. After the wires had been joined, a piece of lead tube of larger gauge encircling the pipe was drawn over the joints and soldered at each end (Fig. 4).

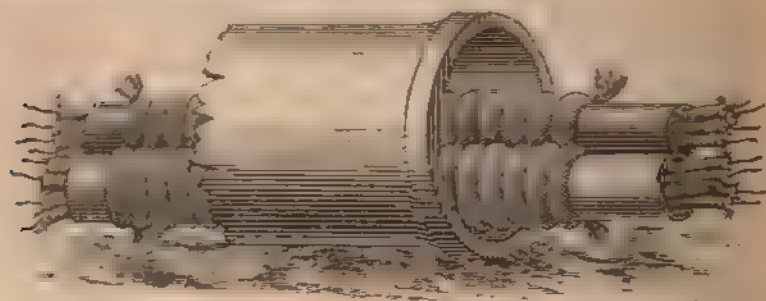


FIG. 4.

Since the commencement of the present year several sections of new 3-inch cast-iron socket pipes have been laid with a view of restoring communication with post offices previously served by over-house wires. One of these sections was from the north-west corner of the Lower Marsh along the Westminster Bridge Road to the post office in Kennington Road. Remembering that the portion of the old line, laid down in 1846, passing through Oakley Street, had not been recovered when the greater portion of the line was taken up in 1852, instructions were given to the lineman in charge of the new work to keep a sharp look-out, and in the event of its being seen, to send word to the office at once. A few days after, I was summoned to Oakley Street, and found that the

old system was still where it had been placed by Mr. Hatcher forty years ago. About three yards were cut out, and when the wires were tested it was found that the insulation was good; the tar appeared as fresh as when first put into the tube. At some future period it will be interesting to recover a greater length of this line and examine the joints both of lead and wire.

For a further description of this early mode of insulation I would refer the members to the remarks of the late Mr. Cromwell Fleetwood Varley in the Society's Journal, No. 12, Vol. IV., page 401, where he commences his remarks by regretting "that very little mention was made of the earliest system of underground work used by the Electric Telegraph Company, and we ought not to lose sight of it, because, should the supply of gutta-percha by any accident fail us, I do not know any other system more promising of success. With the knowledge and the appliances we have at the present day it would be quite possible to lay down wires, not perhaps very highly insulated, but sufficiently so for all the practical purposes of telegraphy." This is followed by a description of the manner in which the wires were covered with the lead tubing, &c. Further reference to this subject will be made later on.

For ten years Messrs. Cooke and Wheatstone—especially the former, who undertook all the practical work of construction—persevered, and in a measure succeeded in overcoming enormous difficulties; for when it is remembered that their wires were insulated with one or more of the following dielectrics—glass, cotton, silk, hemp, shell-lac, resin, bitumen, coal-tar pitch, Stockholm tar, or grease—and protected by either wood troughing, lead tubing, iron troughing, or earthenware, the wonder is how they managed to keep up communication at all. It is not difficult to imagine how faults would be continually springing up; and as all the workmen were ignorant of the nature of the subtle agent that was being used, Mr. Cooke would be compelled (as I understand he was) to turn out himself and trace the cause of the interruption. A lineeman who worked under Mr. Cooke's instructions speaks very highly of his animated zeal and earnestness in the work he had in hand.

Experience teaches us that whenever a special want is felt, a remedy is generally at hand, and only requires to be known to be applied. In this case the remedy was found, but it was being used for making hats, caps, bags, whips, bridles, cordage, boots, piping for syringes, tubes, hose, mouldings for false teeth, and many other purposes. The material I refer to is gutta-percha. From the year 1842, when a specimen was first introduced into England by Dr. W. Montgomery, up to 1845 only a few hundredweights had been exported from Singapore. Since that date many thousands of miles of wire have been covered and buried beneath the surface of the earth, or submerged in the sea, for the purpose of establishing telegraphic communication between London (the chief market of the world) and our provincial towns, villages, the Continent, and, in fact, all places of importance in every part of the globe. The quantity of gutta-percha imported into London alone during the three years ended December 31, 1886, amounted to 6,700 tons. Messrs. Keene & Nickels received a small quantity of gutta-percha from Singapore during 1845, and this appears to have been the first imported into England. This firm ultimately sold their patent for processes in working gutta-percha to the Gutta-Percha Company, Wharf Road, City Road.

During the following two years Mr. Hancock took out patents for cutting, cleansing, and pressing gutta-percha through rollers; but no mention is made of covering telegraph wire in any patent up to the end of 1847.

To Professor Faraday credit has been given of having announced that gutta-percha was an excellent dielectric, and in the beginning of 1848 patents were taken out for covering wire with it. The first patent taken out in England for this purpose, so far as I can learn, was by Messrs. Barlow & Foster, the plan suggested being as follows:—The wire was placed between two heated fillets of gutta-percha, and made to adhere by passing between two rollers. In the same year Mr. John Lewis Ricardo, chairman of the Electric Telegraph Company, patented a machine with a pair of grooved rollers, through which the wires passed, placed parallel between the fillets, the action of the rollers being

to bite nearly through the gutta-percha and allow of the several wires so covered being easily separated.

These early machines did not do their work with the same regularity as those in use at the present time, for it frequently happened that instead of the covering being smooth and uniform, it was found to be thicker at intervals—so much so, that it had to be pared down with a sharp knife.

The directors of the Electric Telegraph Company having discovered that the Strand was not the best position for their chief office in London, resolved to build a central station at the end of Founders' Court, Lothbury. This was formally opened on the 1st January, 1848. From this office a circuitous line of 3-inch cast-iron socket pipes was laid, chiefly under the footways.

Being desirous of connecting the railways and other important places, the route chosen was as follows:—*Via* Princes Street, King William Street, over London Bridge, down the Borough, through Union Street, Charlotte Street, New Cut, Waterloo Bridge Road, over Waterloo Bridge, along the Strand, Whitehall, Parliament Street, Great George Street, Birdcage Walk, Constitution Hill, across Knightsbridge, along the edge of Rotten Row to what is now known as Prince of Wales Gate, across Hyde Park and the Serpentine Bridge to Victoria Gate, Westbourne Street, Spring Street, and thus to the Great Western Railway Station, Paddington. From this main line branches were made to the following places:—

The South Eastern Railway Station, London Bridge.

The London and Brighton Railway Station, London Bridge.

The London and South Western Railway Station, Waterloo.

345, Strand.

The Admiralty.

Horse Guards.

Buckingham Palace.

Exhibition, Hyde Park, in 1851.

Another main line passed up Moorgate Street, Finsbury Square, Pentonville Hill, to the London and North Western and Great Northern Railway Stations. A branch line from this main started at Worship Street, passing through Holywell Lane to the Great

Eastern Railway Station at Shoreditch (now a goods station). A third route was through Gresham Street, Huggin Lane, to the General Post Office. This line was continued to the new office at 448, Strand, in 1852, *via* Foster Lane, Cheapside, St. Paul's Churchyard, Ludgate Hill, Fleet Street, Holywell Street, and along the Strand.

When these pipes were first laid down, wires covered with cotton, passed through one of the preparations in use prior to the introduction of gutta-percha, were drawn into the pipes; but these wires were soon substituted by the wires covered with gutta-percha.

The first section of line in London into which gutta-percha-covered wires were drawn was from Lothbury to Shoreditch, in 1849; and this was followed by the line to Euston and King's Cross. The wires that were first drawn in were covered with gutta-percha by one of the processes already described, and failed, owing to the gutta-percha seam opening longitudinally and exposing the conductor.

At this date a large number of experiments were made: wires of iron, brass, and copper were covered with gutta-percha; in some cases single wire, in others three wires, and as many as seven wires, were enclosed in a solid core. Great improvements were rapidly introduced, and copper wire of No. 16 B.W. gauge was covered with solid gutta-percha of excellent quality up to No. 1 and 3 gauge; and by August, 1854, no less than fifteen miles of pipes, containing 350 miles of wire insulated with gutta-percha, had been put into working order in London alone for the Electric Telegraph Company.

The pipes used by this company were of cast iron, three inches in diameter, and cast with a flat surface on one side, upon which were the initials E.T.C. Across Hyde Park the pipes were of earthenware, the joints being made with clay.

In 1850 the success of the Electric Telegraph Company induced some enterprising capitalists to form rival companies, one of which—the Submarine and European Telegraph Company—laid a line of six gutta-percha-covered wires from London to Dover, *via* Old Kent Road, Deptford, Dartford, Gravesend, Rochester, Chat-

ham, Sittingbourne, Canterbury, and thence to Dover (Fig. 5).

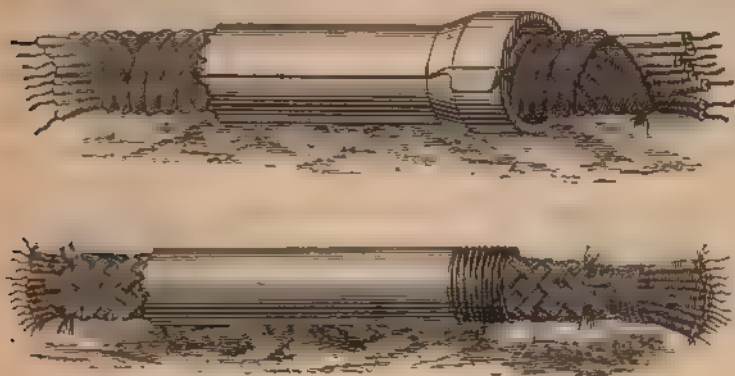
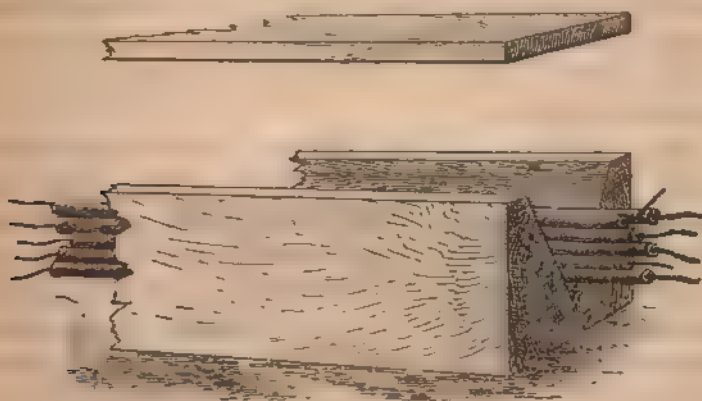


FIG. 5.

This line was completed by the 1st November, 1852. The following year—1853—was a very busy one for the manufacturers of gutta-percha-covered wire, for in addition to the line of eight wires put down by the Electric Telegraph Company on the London and North Western Railway from London to Manchester, the British Telegraph Company, which obtained Parliamentary powers in 1850, commenced burying six gutta-percha-covered wires along the high roads from London to Liverpool, *via* Birmingham and Manchester, the latter place being reached by the 1st of March, 1854, and Liverpool during the same year (Fig. 6).





The Magnetic Telegraph Company, of which our President was the distinguished engineer, was formed in 1850, and in 1853 took steps to gain the favour of the commercial world by providing rapid communication between London and the provinces, also to Ireland; the route selected was along the main roads, similar to that of the British Company (Fig. 7). Ten wires were



FIG. 7.

laid as far as Liverpool, passing through Birmingham and Manchester, and six wires between Liverpool and Portpatrick. Underground wires were also laid between Cork and Queenstown.

After a very short time these companies united under the title of the British and Irish Magnetic Telegraph Company, which became a very keen rival to the Electric Telegraph Company. There was so little difference in the manner in which these companies constructed their lines, that I do not consider it necessary to describe them separately. Speaking generally, they buried a wooden troughing of about three inches outside, with a groove about one inch square, having a lid of the same material; this was fastened down with nails after the wires had been placed in the troughing. The iron troughing was of about the same dimensions externally, but, of course, allowed more space for the wires inside; the lids were of iron.

When cast-iron pipes were used in towns, they were very similar to the iron troughing, being cast in two pieces; they were patented by both Mr. Reid and Mr. Henley, and are known as Reid's and Henley's split pipes. The wires were in some instances laid in troughing without any protection beyond the gutta-percha, but in other cases they were protected by two layers of tarred yarn laid on in opposite directions. The whole of the stores appear to have been of the very best quality.

The underground line of the Electric Telegraph Company

laid along the side of the London and North Western Railway from London to Manchester and Liverpool was somewhat different from those just described. The pipes were of earthenware, the joints being made with clay. At intervals of fifty yards split earthenware pipes of larger diameter were fixed, to allow of the wires being drawn in. An iron wire was passed through the pipes as they were being put together, for the purpose of drawing the cables through.

From London to Watford four wires only were drawn in, but these had to be increased to eight, which number was continued on to Liverpool.

The work of constructing these different systems was finished by the beginning of 1855, and, after a very short life, in 1857-58 they were condemned, and wires on poles substituted. This applied to all the underground lines laid from London to the provinces in 1853.

In the *Engineer* of December 23rd, 1859, the failure of these lines was attributed to the gutta-percha. The writer says: "The real truth is that gutta-percha has been tried as an insulator, and failed—signally failed. That it has failed for subterranean telegraphs is beyond all doubt; that thousands of miles of gutta-percha-covered wire have been laid underground only to be dug up again within a comparatively very short period is notorious, and if any of your readers have the slightest doubt upon the matter, a visit to the Electric Telegraph Company's store at Camden Town will convince the most sceptical. The expense of all this to the telegraph companies has been enormous. The Magnetic Telegraph Company, for instance, began their lines, principally underground, at an enormous original cost; before completion they were liable to interruption through bad insulation, and in two or three years nearly the whole had become totally bad; some were replaced by fresh wires, only soon to go again in the same way, until at length they were obliged to abandon portions of their lines and erect pole telegraphs upon the highways; and if your readers will take into consideration that a line of six gutta-percha-covered wires cannot be laid

"underground for much less than three hundred pounds per mile, they may form some idea of the frightful losses the telegraph companies have sustained through the failure of gutta-percha as an insulator: and so far has this failure gone, that it has become a profitable thing on the part of the gutta-percha manufacturers to buy up in 1859, for £6 and £7 per mile, covered wire sold in 1857 for £18 and £20 per mile, strip the gutta-percha off the wire, and use both gutta-percha and the wire again—very reproductive, indeed, and a novel mode of keeping up the supply of gutta-percha."

Now, as the writer of the above was a very strong advocate for india-rubber for insulating subterranean wires, it will perhaps be better for us to look elsewhere for an explanation of the premature decay of these early undertakings, rather than accept his wholesale condemnation of gutta-percha as an insulator. When the greater portion of the wires were taken up in 1857 and 1858, a few sections were allowed to remain, especially in and near London. Portions of these lines have been recovered at different periods, in different localities, and under various circumstances. Some of the gutta-percha wires that have been buried for more than thirty years are now in first-class condition, but where this is the case they have been found wrapped in two layers of tarred yarn, and laid in iron casing or split pipes. The same cannot be said of that found in wood troughing, except under special surroundings. There is every reason to believe that the gutta-percha used to cover these wires was the real material, and was not subjected to anything akin to the so-called improvements in manufacture of late years.

I have frequently been amused to see the expression of satisfaction on the faces of our linemen—men well acquainted with the peculiarities of gutta-percha—when they have come across one of these old lines, and subjected the material to certain ready tests, exclaiming, "It's splendid!"

Although the insulating material was good, yet there were many faults in the wire previous to its being buried. Probably owing to some fault in the machinery used for covering the wire, the copper wire got elongated and pressed in the form of a loop

through the gum; many such places were discovered when the wires were recovered and examined.

The wire used by the Electric Telegraph Company was covered with gutta-percha, and then each wire was served with tape. After this wire was delivered on the railway at Watford, it was passed through a bath of hot tar and sand: while the drums were moving at a regular pace, a serviceable jacket of tar and sand was given to the wire, but when anything happened to interrupt the continuity of this operation the portion left in the bath was exposed to a temperature sufficient to melt the gutta-percha, and the wire passed out covered with tape, tar, and sand only. Adequate care was not exercised in protecting the wire from the heat of the sun, it being allowed to remain lying on the banks of the railway uncovered for a considerable period, so that the copper wire got out of the centre, and in some cases became exposed altogether. After the cables had been put in place, the drawing-in holes at every fifty yards were left uncovered, and the ends of the wires left exposed, waiting for the jointers to come and make the permanent joints, and the split couplings to be put on.

The means adopted to preserve the wires were defective. Knowledge derived from a series of observations points to the fact that split pipes, whether of wood or iron, are not equal to the requirements of a subterranean line of telegraph wires. Wood troughing is subject to dry rot, and therefore various expedients have been resorted to with a view of preserving the timber; but some of these preservers act injuriously upon the wire, causing the insulation to be destroyed. When the line is placed near to trees, especially oak trees, the surrounding moisture is absorbed for a considerable distance; this applies to lines constructed of either wood or split iron pipes. Special circumstances may arise that would make it expedient to use wooden troughing, but where there is an absence of such special conditions solid cast-iron pipes are to be preferred. In London split pipes are a source of danger, for since the chief thoroughfares have been paved with wood or asphalt, the gas that escapes in considerable quantities from the gas companies' mains accumulates under the surface and finds its way into these half-pipes, and thence into our junction-boxes,

causing much annoyance and inconvenience. I have known cases where the accumulation of gas collected through split pipes has been so great that it has been necessary to open the box some minutes before the jointer could light his lamp with safety.

Where earthenware pipes have been used instead of iron, it has been done for economical reasons only. Such a line was laid some years ago from opposite St. George's Hospital, Knightsbridge, across Hyde Park, to Tyburn Gate, the joints being made with clay. Another section of the original line laid in 1851 between the site of the Exhibition of that year and the Serpentine Bridge was of earthenware. Some years ago the sixteen wires that were drawn in when the line was laid, had to be drawn out to allow of the number being increased. Much difficulty was experienced in moving the old wires, and it became necessary to break one and by laying it over the trench so to trace the locality of the obstacle; when this was done, and the ground opened, it was found that the root of a tree, about ten feet long, had found its way through the clay with which the joints of the pipes were made, and had entwined itself round the cable in such a manner as to hold it fast. I need scarcely say that the gutta-percha had entirely perished at this point. Pipes of this description are always more liable to be damaged by the operations of the gas, water, or the hydraulic power companies' workmen when opening ground to get to their mains, than solid cast-iron pipes.

Another, and I think the chief, cause of the rapid decay of the lines referred to, was the rapidity with which the work was carried out. All the companies were racing to get through to the provinces as soon as it was possible, and there is no doubt that this led to the work being done without the necessary supervision being given.

At the present time it is found necessary to place experienced men to watch the work as it proceeds, for where this has not been done the work has proved to have been unsatisfactorily carried out. When this is taken in connection with the fact that the majority of the men employed on these works were totally ignorant of the necessity of the greatest care being exercised, and the utter impossibility of the few able men to be everywhere

along the works at the same time, it is not surprising that these early lines had to be so soon abandoned. Although the loss sustained by the different telegraph companies, through the failure of these underground systems, must have been enormous, yet we find that in 1861, when the "District" and the "United Kingdom" Telegraph Companies started, they both began by putting down subterranean wires. The first-named company chiefly employed seven gutta-percha-covered wires, laid up with a slight twist, encased in a tube of the same material filled with a preparation known as "Hughes' fluid," but unfortunately these excellent cables were laid in either wood or iron troughing as already described.

Another kind of cable employed by this company was one patented by Mr. E. Highton in 1850. Seven gutta-percha-covered wires were laid up in a strand and surrounded by a flexible covering of iron wires. In some instances this cable was buried under the footpaths and roadways without any other protection whatever. It was thus liable to be injured by the first gang of workmen that had occasion to open a trench across its route. A long length of this cable still lies buried in different parts of London, but the majority of the sections are out of use through mechanical injuries that have been occasioned in the manner alluded to; many faults have been traced and cut out. The plan adopted by the United Kingdom Telegraph Company consisted chiefly of gutta-percha-covered wires in split pipes laid under the footway from Gresham House to the Canal Wharf Road.

We now come to a very important epoch in connection with the telegraph systems of this country—the transfer of the telegraphs to the Post Office. This took place in the beginning of 1870, when all the different companies' lines were diverted to the central office of the Electric Telegraph Company in Telegraph Street, Moorgate Street. This work was carried out under the direction of Mr. Henry Eaton, now the superintending engineer of the metropolitan district.

After the concentration had been effected it was soon discovered that nearly all the lines, with the exception of those in



solid cast-iron socket pipes, were defective in insulation; and as the various methods of construction did not provide for renewals to be carried out without opening the ground, they were gradually abandoned, and the number of wires in the 3-inch cast-iron socket pipes of the late Electric Telegraph Company increased to meet the ever-expanding requirements of the service.

Previous to the completion of the new Post Office in St. Martin's-le-Grand, in 1874, the pipes and wires passing north, south, east, and west of the new building were led through the basement and up to the instrument gallery, where they were terminated upon a very extensive test-box, the terminals being numbered from 1 to 1,000.

The contemplated change from the office in Telegraph Street having upset all the numbers in the joint-boxes in the streets, it was considered necessary to re-number the whole, and thus make the numbers in the boxes correspond with the new test-box. This was carried out so perfectly that the whole of the circuits were diverted to the new office in a very few hours on the night of January 17th, 1874, without the slightest interruption to the ordinary traffic.

At this date the length of pipes in the metropolitan district was about 100 miles, and the total quantity of wire in the pipes amounted to 3,000 miles. Shortly after this date an agitation was started against the over-house wires, and it was decided by the Post Office authorities that a new line of 3-inch cast-iron pipes should be laid down, and 100 miles of wire drawn into it, in substitution of 126 miles of over-house wire.

This was the first piece of work carried out in London with a view of reducing the over-house system belonging to the Post Office, and this was supplemented by many others, until the whole of the main lines of over-house wires had been removed.

The chief portion of this work was finished before the introduction of that marvellous little instrument the telephone, which must be held accountable for the dangers apprehended from the extraordinary network of wire suspended over the chief thoroughfares of the City, and to a lesser extent the suburbs of London. It has also had the effect of largely increasing the



mileage of underground wire, for it was soon found that for a telephone to work satisfactorily underground, two wires must be used instead of one, and after many experiments it was found that much better results were obtained by the use of four-wire twisted cable covered with tape, two of the wires being used diagonally for the telephone circuit, thereby overcoming the effects of induction. Twenty of these four-wire cables, equal to eighty conductors, are now generally drawn into one 3-inch cast-iron pipe (Fig. 8), some of which form part of the original lines laid

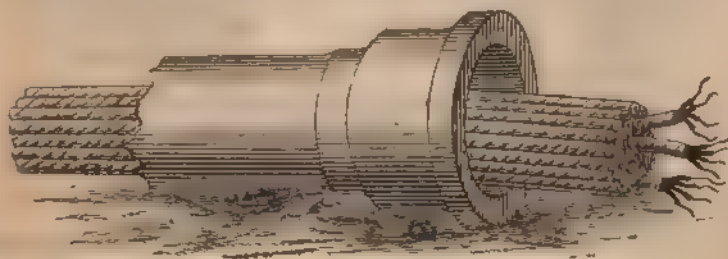


FIG. 8.

down in 1847—forty years ago; and judging from what I have seen when the wires have been drawn out for increasing, there is no reason to think they are likely to give any trouble for many years to come.

At the beginning of 1880 an experimental line of 1½ inch wrought-iron pipe and 30 No. 18 B.W. gauge copper wires, each covered with manilla, and the whole made up into a cable and covered with a braid of the same material, was laid for the Postmaster-General, under the direction of the patentee, Mr. David Brooks, of Philadelphia. The route chosen was on the viaduct of the London and South Western Railway from the Westminster Bridge Road to where the line passes over Bond Street, Vauxhall. It was subsequently extended to Clapham Junction Station, 30 wires being continued on to Queen's Road Station, and 40 from that point to Clapham Junction Station (Fig. 9).

The peculiarity of this system consists in the mode adopted for insulating the wires. The cable, previous to being drawn in, was coiled in a large cauldron and covered with paraffin oil; a

fire was then made under the cauldron and the contents heated to  $300^{\circ}$  Fabr.; the fire was then checked so as to maintain that degree of heat for one hour, to give time for the heat to permeate



FIG. 9.

thoroughly through the body of the cable. It was then attached to an iron wire which was threaded through the pipes when laid, for the purpose of drawing it in; previous to commencing this operation a quantity of hot oil was poured into the pipe, and as the cable was being drawn through, a man stood pouring hot oil over it through a funnel attached to the end of the pipe, as it passed out of the cauldron and entered the pipe. The joints of the pipes were made by means of stout sockets and a solution of shellac, which is supposed to make the joints impervious to oil, the latter having no affinity for shellac. To keep up a constant supply of oil a reservoir was fixed at a slight elevation on the bridge over the Westminster Bridge Road and connected to the pipe.

After this line had been completed, the wires, with one or two exceptions, gave very good results as regards insulation, but from the first there was a very great loss of oil, and this continued more or less although every effort was made to trace and repair the leaks. Probably the constant vibration of the viaduct has had something to do with the leaky condition of the joints. Owing to the viaduct being widened for a considerable length, it was necessary last year to divert the wires working through this system between Westminster Bridge Road and Queen's Road

Station, and to recover the pipes and wires along the railway between the above points. This was done, and the remaining portion from Queen's Road to Clapham Junction thoroughly overhauled, one section of about 300 yards being renewed. Since last October the line has worked well, and although the cost of construction and maintenance has been very heavy, it does appear to me that the system is worth a further trial under more favourable conditions—for instance, along a country road where it might form part of a through line, and where the pipes would not be subjected to such frequent disturbances as in the busy thoroughfares of London.

Several very large extensions to the boundary of the metropolitan district, and in some cases even beyond, as well as the enormous increase that has taken place in the supply of wires for private use, have increased the mileage since 1874 up to the present time from 100 miles of pipes and 3,000 miles of wire to 240 miles of pipes and 12,000 miles of wire, the latter being an average of 50 wires through the entire length of pipes. The total weight of copper buried in the metropolitan district amounts to over 200 tons, and that of gutta-percha to 250 tons.

The underground lines radiating from the General Post Office to the provinces extend to the following points, thence to join wires on poles running along either the railways, highways, or canals:—

## RAILWAYS.

Ludgate Hill Station	...	...	...	}	L. C. & D. R.
Elephant and Castle	...	...	...		
Bromley Station	...	...	...		
Waterloo Station	...	...	...	}	L. & S. W. R.
Queen's Road Station	...	...	...		
Clapham Junction Station	...	...	...		
Raynes Park Station	...	...	...		
Kew Bridge Station	...	...	...	}	L. & N. W. R.
Isleworth Station	...	...	...		
Euston Station	...	...	...	}	M. R.
Loudoun Road Station	...	...	...		
Finchley Road Station	...	...	...		

Maryland Point Station	...	...	} G. E. R.
Leyton Junction	...	...	
River Lea, Tottenham...	...	...	
King's Cross Station	...	...	} G. N. R.
Holloway Station	...	...	
Paddington Station	...	...	} G. W. R.
Hanwell Skew Bridge	...	...	
Brentford Station	...	...	
London Bridge Station	...	...	} S. E. R.
New Cross Station	...	...	
London Bridge Station	...	...	} L. B. & S. C. R.
New Cross	...	...	

## ROAD LINES.

Bath Road, Hounslow	...	Bristol Road Line.
Denham	...	London to Carlisle and Scotland, <i>via</i> West Coast.
Wanstead Flats	...	Ipswich Road Line.
South Mimms Road, Barnet...	...	Birmingham Road Line.
Hadley Green, Barnet	...	London to Newcastle and Scotland, <i>via</i> East Coast.
Tottenham, near G. E. Ry.	...	Cambridge Road Line.
Bromley Common	...	Beachy Head Road Line.

## CANAL LINES.

Harrow Road, Paddington	...	Canal and Road <i>via</i> Oxford.
Do.	do.	Canal and Road <i>via</i> Northampton and Nottingham.

Other points to which underground lines have been laid for Post Office and private wire services:—

Kilburn.	Victoria Docks.	Fulham.
Hampstead.	Woolwich.	Hammersmith.
West India Docks.	Blackheath.	Brixton.

It will be obvious that in the main thoroughfares near to the General Post Office the wires are very numerous. Through Little Britain there are two 3-inch pipes containing 109 wires; Aldersgate Street, two pipes, 160 wires; Gresham Street, two

pipes, 150 wires; Cannon Street, two pipes, 168 wires; Cheapside, two 3-inch, one 4-inch pipes, and 242 wires; Ludgate Hill, one 4-inch, three 3-inch pipes, 350 wires; and down Newgate Street, two 4-inch, three 3-inch pipes, with over 400 wires. These numbers have been reached by gradual increments, the practice being to put into a new pipe as many wires as are actually required for the special service, with a small percentage of spare wires, and as soon as these have been appropriated for new circuits, to increase the number by drawing out the existing wires and replacing them by a cable with additional conductors. This class of work is continually being carried out in different parts of London to meet the great increase in telegraphic and telephonic business.

During the last fifteen years nearly every section of line has been disturbed, either for the purpose of removing faults which sometimes trouble us in or near the flush-boxes, through the necessarily frequent exposure of the gutta-percha to variations of temperature caused by the linemen getting new wires through, &c., or by the necessity of increasing the number of conductors as occasion may arise. So much of this work has been done that it would be difficult at the present time to point out any portion of line that has not been renewed during the above period.

The advantages of being able to replace the working wires by a greater number without seriously interrupting the communication must be evident to all; but this is easily accomplished where the lines are constructed of 3-inch cast-iron socket pipes with boxes either fixed flush with the pavement or buried beneath the surface at every fifty or one hundred yards, according to the size of the cable to be drawn in.

After many years' experience, and with a full knowledge of the many schemes that inventors have introduced from time to time, I do not know of any that can be compared with the present system for simplicity, utility, and durability.

On being asked by the Chairman whether he wished to add anything to the paper,

Mr. C. T. FLEETWOOD said: I merely rise to point out that in <sup>Mr.</sup> ~~Electronics~~

Mr.  
Fleetwood.

the paper I have made a remark to the effect that it would probably be interesting at some future time to recover a portion of the old line that was laid between the Strand and Nine Elms. The portion referred to was only some 3 yards long. About a fortnight ago I recovered 70 yards, and on testing it found it to be faulty. I cut it up, and succeeded in getting two lengths of 21 yards each, free from earth or contact, and a portion is now before the meeting. I thought I would mention that, as it is not in the paper.

Maj. Gen.  
Webber.

Major-General C. E. WEBBER: There are many points in connection with this most interesting paper which must come home to the minds of those who have at times in their life been engaged on that interesting and excellent work upon which the greater part of Mr. Fleetwood's career has been spent.

As regards the question of gutta-percha, I think we must not forget that the difficulty of obtaining sound gutta-percha is enormously enhanced by the demand having caused, during the last fifteen years, adulteration of that article to a very considerable degree. About the year 1874 I remember examining a line which was laid down between the end of the overhead line to Lowestoft and the cable huts on the sea shore, in which the gutta-percha had then been in the tube for between eighteen and nineteen years, and the gutta-percha which was recovered was positively, as far as one could ascertain by the ordinary means of testing at our disposal, as good as new. No such gutta-percha as that is obtainable at the present time.

In the year 1878 I went to Amsterdam for the purpose of examining the gutta-percha then in the market, it having been suggested to me by one of the wire coverers in Paris, and I found that there were there over fifteen samples, varying from £10 to nearly £100 a ton in value, and that no sample that could be bought by wire coverers, except those who were engaged very largely in the trade, could be obtained that was not a mixture of some five or six of them, some being, of course, of a very inferior character. I have no doubt that this has been going on since then, and that the insulation of telegraph and telephone conductors has been subject to this source of deterioration, and has,

to a great extent, added to the troubles of the telegraph-engineer, and has brought into discredit a good deal of the enterprise of many of the undertakings in which such wires have been used.

I think Mr. Fleetwood, in his most interesting paper, has not laid quite sufficient stress on the importance of, I would not say reforms, but would call them the completion of the system, or the bringing into perfection of the system, of underground electric telegraphs about that epoch, which he mentions, when the telegraphs of this country were transferred to the service of the Post Office. And, while mentioning my old friend Mr. H. Eaton, I would also like to mention his assistant, Mr. Shipp, a man who was well known then in the streets of London in connection with underground work. Also I think we must not forget to give credit to the then engineer-in-chief of the Postal Telegraphs, Mr. R. S. Culley, who, I believe, was chiefly instrumental in bringing that system into the state of perfection which it then had attained, and of which I am sure it has lost nothing under his able successor. But the whole gist of that system, or the fundamental rule for the guidance of laying down and maintenance of that system, was the complete power and facility in the hands of the engineers and men maintaining it, of withdrawing a cable at a moment's notice when it was required to be removed either for renewal or repair; and it is due to that alone—you may say almost due to that guiding rule—to which the Post Office and all those who have been engaged in the work owe their great success, because it has neutralised the effects of that deterioration of the insulator which was unavoidable, and which would otherwise have brought them into an immense amount of trouble and difficulty. I have often said to myself, when maintaining and having to do with underground telegraph lines, that the man who had the responsibility of such maintenance ought not to be able to sleep if he could not send men down at a moment's notice to draw out a section and replace it in a very few hours. I cannot help dwelling on this subject with reference to telegraph and telephone lines, and also to other electrical conductors, and suggesting what often comes into my mind now, as I see our city being day by day covered, not only by small

MAJ. GEN.  
WEBSTER.



1884.  
 1884.

wires, but also by large ones—structures that look more like the main anchor cable of a ship than anything else—and it is this, that if our capitalists—men who have in former years put their money into such lines, with far less engineering knowledge as to their construction and maintenance than we have now—if they would only foresee that the day must arise when all these conductors will have to be placed underground, and if they would not mind postponing the result—it is very difficult to get people not to mind laying up their capital—but if they would commence now, with the view of in three, four, or five years hence having a perfect system of passages, ways, or tubes under the streets and lanes of London, and elaborate their system so as to provide, amongst other things, for, say, a hundred telephone exchanges in London in ten years hence, and would lay up their capital in such a system, then, at the end of that time, when the public would no longer bear these spider webs over their heads, the preparation would be ready, and this great city could be provided with a system with which nothing could compare in this world. Such a system must be carried out, not, as Mr. Fleetwood said was done in the early days of the telegraph companies, hurriedly, but cautiously, carefully, and without interference with the public convenience. It must be done well beforehand, and then we may look forward a few years hence, when the telephone patents have ceased to control the market, and when electric lighting takes its proper place, to be able to establish such a system of electrical conductors in London as will meet the demands for electrical distributions for telephony, for light, and for power, such as that wonderful agent of the purposes of man has a right to claim, and to which we, its engineers, believe it to be entitled.

r. Adams.

Mr. A. J. S. ADAMS: We should, I think, be careful how we repeat history, unless it be for the purpose of pointing a moral or of adorning a tale. The paper we have just heard is wholly history, and although the author has compiled it with care, and has adorned it with interesting specimens of gutta-percha work, most of us, I fear, do not exactly see the moral. We need something more than mere history: for instance, the author, in a paper read before this Society in 1875, stated that a large portion

of the Post Office London street work had been then renewed, Mr. Adams.  
and we would like to have learnt the practical result of that work. Much was also said in that previous paper upon the deleterious effects of gas in the pipes, and of attempted protection by hermetic sealing—a matter to which the present paper makes no reference. In 1873 Mr. George Preece gave an interesting account of a new gutta-percha line that had been laid down between Liverpool and Manchester, and here again it is strange that in treating of underground work by the Post Office method, no reference has been made to that line. One would have thought that the consideration of underground trunk lines would obtain a prominent place in this paper. The importance of this question of underground work becomes more prominent every year, and especially after such a collapse of open work as that experienced during the past winter. I know that some people say there is no hope of maintaining continuous telegraph working unless all lines are put underground, and that there are other people equally certain that it would never pay to take such a course. But, Sir, in regard to these periodic disturbances to telegraphic communication by snowstorms, it seems to me that the Clerk of the Weather is held responsible for a great deal more than he deserves. The fact is that, in order to pay dividends, both construction and maintenance are starved, and when we remember the enormous profits obtained from telegraph wires the state of things frequently existing becomes indefensible.

There would be no necessity for putting wires generally underground if the open work were fairly treated, and I am confident that open work may be put up and maintained at a fair cost to withstand any storm that may try it.

Then there is the question of the practicability of underground trunk lines, to which I would briefly refer, because I think that from a gentleman of the large experience possessed by the author of the paper before us we might have expected valuable expressions of opinion, and that the sphere of his remarks would take a wider range than that embraced by the circle of "Suburban London." I have no doubt—as was indicated by Major-General Webber—that the supply of gutta-percha would prove quite

Mr. Adams. inadequate for the purpose of trunk lines; and the question arises, What is there to supply its place?

I will not trouble you with my own opinions regarding the best method of utilising asphaltum—my opinions upon that point are already on record—except so far as to say that in asphaltum we have one of the finest materials for the purpose, and that as an insulator for our conductors it has a great future before it. One thing is certain, and that is, that asphaltum has never had a fair trial in underground work; and I trust the author will add his opinions upon this and the other points raised to his interesting historical essay.

Mr. Preece. Mr. W. H. PREECE: The chief merit of Mr. Fleetwood's paper is that it has brought before the Society the result of his own practical experience; and the only recompense that we officers of the Post Office can pay to those who support us is to give them freely, clearly, and voluntarily, whenever we have the opportunity, the results of our experience. Mr. Fleetwood has confined this paper to his own experience, and therefore he has not referred to lines like the Liverpool and Manchester underground line, that is not within his own experience. But, as this line has come within my experience, I can answer Mr. Adams's question, and I can tell him that the underground line that was laid down between Manchester and Liverpool in 1870 lies down there still, and will continue to lie down there as long as he and I remain on the surface of this globe. It has succeeded in maintaining through all weathers, and in all circumstances, uninterrupted communication between those two great centres of commerce, Liverpool and Manchester.

It was laid down in earthenware pipes, and it has been found by experience that it is almost impossible to maintain intact, however well they may be made, the joints in such pipes; and the result has been that, owing probably to expansion and contraction, to the subsidence of the ground, and to other causes, those joints have cracked, roots, branches, and vegetation of various kinds have made their way into the pipes, and the result has been that a large portion has been replaced by iron pipes.

There are two great historical points of interest that have

been brought before us by Mr. Fleetwood. First, that in the year 1823 Mr. Ronalds wrote a paper on underground telegraphs that would do credit to any member of this Society if written in the year 1887. It is perfectly astonishing how that man's instinct saw the various troubles that were likely to be met with in the construction of long underground lines. The paper is to be found in the Ronalds Library; it is a pamphlet that is well worth studying by everybody here. Secondly, Mr. Fleetwood has referred to the works of the late Mr. Cromwell Varley. Now I do not think we should ever forget the fact that nearly all the advances that have been made in practical electricity were really due to the insight and instinct of Mr. Cromwell Varley. As far back as 1848 he commenced to test the very wires of which specimens are here before you, laid down between the Strand and Nine Elms; and while, before Mr. Varley's day, people used to cut up the lead wire to find a fault, he commenced with his galvanometers and resistance coils to test the distance of faults by the resistance of the conductor, and from that day commenced that immense advance in our knowledge of electricity, when practical men began to learn the necessity of applying scientific methods, and learned how to measure in absolute units of resistance, electromotive force, and currents.

Gutta-percha has been referred to by Major-General Webber, but nobody has referred to copper. There has been just as much advance made in the quality of the copper used for our underground and our submarine cables as in any other branch of telegraphy; and while, curiously enough, during the past thirty or forty years we have done everything we can to improve the purity of our copper, we have, as General Webber remarked, been deteriorating the quality of our gutta-percha. Now that is probably due to the fact that the demand has exceeded the supply, and that it has been impossible, in the markets of the world, to secure sufficient pure gutta-percha to meet the demands made upon it. The result has been that impure stuff from the West Indies and every other part of the world has been brought in and has been palmed upon us as gutta-percha, and the result has been that the quality of the material that we have used until

Mr. Freese. recently has very much deteriorated, and we have suffered in consequence. But, in spite of all that, we stick to gutta-percha. We have used gutta-percha now for over thirty years; we know its life and its peculiarities; we know what it can do, and we know what to do with it. We have tried everything else that has been brought to the front. We have tried india-rubber, and it has failed. India-rubber is admirably adapted—is the only material, in fact—for indoor purposes; but when you come to underground and submarine cables, then india-rubber utterly fails, and gutta-percha has been successful. Mr. Fleetwood has referred to an article in *The Engineer* some thirty years ago. I remember that article very well, and I remember the author of it. The author of it was a Mr. Charles West, whom we all called “India-rubber West;” he had only one idea in his mind, and that was, that there was nothing like india-rubber. Well, then, bitumen has been referred to. Bitumen in various forms has certainly been tried. Some—I am almost afraid to say how many—years ago Mr. Donald Nicoll introduced or started a mode of laying underground wires by means of bitumen. It was tried, but for some reason or other Mr. Nicoll did not succeed; and I see he is active at the present day in the matter, and only a few days ago he read a paper on the subject. Perhaps some day he may favour us with his views. But bitumen has gained enormous strength in the results of Mr. Fleetwood’s inquiries. He has brought up some wire that he has referred to which is very well worth your notice, because you will find that this lead-covered wire that has been underneath our streets, unknown and forgotten, for forty years, and has now, with Mr. Fleetwood’s interest, been brought to the surface, and which has been examined, is absolutely perfect. It has all the smell of pitch or tar; the yarn with which it is covered is as sound as it was the day in which it was put there; and certainly it is one of the most hopeful things that we have had.

Then Mr. Fleetwood has also alluded to oil. There is a new process recently introduced which, to my mind, is very promising indeed. We tried, as Mr. Fleetwood described, Brooks’s system on the London and South Western Railway; it has been tried in America. It has not failed, but it has not been very

successful. I need not enter into the reasons of its partial Mr. Proce. success, but they have been very obvious. But the new material introduced by Mr. Eddison—not *the* Edison, but a partner in the firm of John Fowler & Co., of Leeds—which he has brought over from America, is a process of covering wires with lead, the wires being insulated with some such material as Brooks used—i.e., a combination of resin oil and resin. Brooks used only oil, but in this new process resin oil is mixed with resin, and the material is solid, not liquid as in Brooks's system. It has a most remarkably high insulation; some samples that Mr. Kempe tested a short time ago gave a result of 19,000 megohms per mile, which was so high that I had some doubts of its permanence. The first test was made in the early part of the year; last week Mr. Kempe tested it again, and again the same material gave exactly 19,000 megohms per mile. Well, I think when we get materials with such a high insulation as that, with sure indications of its permanence, there is some hope that we have something that will make us independent of fraudulent gutta-percha.

It has been said more than once that Mr. Graves and myself, and others of the Post Office, are determined opponents of underground work. We are not. The fact is that at the present moment we have more underground wires in England than any other country. In Germany, up to this year, they have been in front of us. At the commencement of this year Germany had 22,000 miles of underground wire; we had 19,000 miles. At the present time we have very nearly 25,000 miles of wire underground, and therefore we have gone ahead of Germany, and we shall go ahead further and further. We are very strong believers, and always have been very strong believers, in underground work; but there are very good reasons why we have opposed the indiscriminate transfer of our wires from open poles to underground pipes. Whenever a snowstorm takes place of the nature so destructive to overhead telegraphs—and such snowstorms take place only about every ten years—we can afford to see our wires broken down and hanging about our heads, providing the interruption only lasts for twenty-four hours, for we have so many alternative routes. But if the interruption were to last for months, why, then, the matter

Mr. Preece. would be rather more serious. We have certainly come to this conclusion—that there would be an advantage in placing some of our trunk lines underground; and at the present moment there is a proposal before the Postmaster-General that will probably be submitted to the House of Commons, asking for a large sum of money in order to put more wires underground. I do not think the House of Commons will agree to spend two and a half millions on our telegraphs; but, nevertheless, there is a proposal to that extent afoot; and if they will allow us to spend that money we shall be able to place our system in a position, as regards trunk lines of communication, perfectly clear from snowstorm damage. Our strong objection, however, to putting wires underground is this—that while a wire underground costs us, or has cost up to the present moment, four times as much as a wire overground, it also is much worse commercially: it only has one-fourth of the carrying capacity for the transmission of messages; i.e., while you can work a long overhead wire at the rate of 400 words a minute, you can only work an underground wire of the same length at the rate of 100 words a minute, and, therefore, to put a wire underground means commercially a loss of fifteen-sixteenths of the value of a wire; that is, a wire underground is only commercially one-sixteenth the value of a wire overground, because it costs four times as much and has only one-fourth the capacity. But that ratio may probably with further experience change. Some of those new materials and processes may become practical and cheap. The Callender's Company have introduced bitumen very largely for electric light purposes, and very promising stuff it is indeed. There is a hope that the cost will be brought down to, say, twice the cost of overground wire. I had the pleasure not very long ago of pointing out to you that the rate at which signals were transmitted through a wire, was dependent upon the product of the total resistance into the total capacity of the circuit. Now, if we can reduce the total resistance by the use of purer and thicker copper, and if we can reduce the total capacity by using material like bitumen and resin oil—which have a much less specific inductive capacity than gutta-percha—then if we reduce the price one-half, and if we increase the message capacity twice, we shall bring the commercial value



of underground lines, instead of one-sixteenth, as it is now, to Mr. Proce. about one-fourth.

The value of these papers from telegraph men who have had forty years' experience is not to be ignored. The value of the paper is this—that whether you apply these systems over such long experience to the delicate currents required for telephones, or whether you use them for the much more powerful currents required for electric lighting—and the experience is exactly the same—the different faults that telegraphic or any other currents develop, the comparative merits of the different pipes, ways, or troughs laid down, and of the various joints that are made, are all learned from the lessons of the past. So, whether it be those who are contemplating the distribution of electricity by direct currents or by the use of very high tension indeed—and the use of secondary generators is coming to the front every day—these lessons of experience are equally valuable.

Mr. ALEXANDER SIEMENS: I want to say just a few words Mr Siemens. about the historical aspect of the subject. In 1845 or 1846 Dr. Werner Siemens asked that some gutta-percha might be sent to him to Berlin, because he had heard of Dr. Montgomery's communications, and believed it would be a good insulator for telegraphic purposes. In consequence of his experiments he laid in 1846 an underground line between Grossbehren and Berlin. This line failed for the same reasons as the English lines failed, the covering of the wires being done in an unsatisfactory manner, and the gutta-percha not carefully enough selected.

In 1847 Dr. Werner Siemens constructed gutta-percha presses which in principle have been used up to the present day in nearly all factories where gutta-percha wires are made. At the meeting of this Society on the 23rd February, 1876, Dr. C. W. Siemens went fully into this matter, his remarks being recorded in the Journal, Vol. V., page 81. The question of deterioration of gutta-percha has, of course, come to the notice of our firm as much as to anybody else's, and that has induced us to study the subject very carefully, and try to remedy it, and we have succeeded in producing from the present "mixed" gutta-percha a product

which is not surpassed by any of the old gutta-percha. I would only remind the meeting of what most of them know, probably—that the six Atlantic cables which have been manufactured by Messrs. Siemens Brothers are the longest cables worked on the duplex system, and also that the inductive capacity of this improved gutta-percha is much lower than that of the best old gutta-percha.

Mr. R. W. EDDISON: Mr. Chairman and gentlemen,—I have very great hesitation in rising in the presence of so many persons who thoroughly understand the subject of electricity and telegraphy; but as Mr. Preece has called upon me I have pleasure in giving a short description of the cables to which he has referred, and which are now being manufactured by my firm at Leeds. During a visit I made to America a year ago my attention was drawn to a new system of covering electric telegraph and telephone cables with lead—the invention of a relative, Mr. James Tatham, of Philadelphia. I had special opportunities of examining the manufacture and working of these multiple cables, and satisfied myself that they were extremely good, and had considerable advantages over many that had been used hitherto. I had some cables tested, and found them exceedingly high in insulation and decidedly low in inductive capacity. I brought several to England, and the tests made here by an eminent authority on electrical testing confirm in every respect the tests made in America, and I am sanguine that they will aid in surmounting some of the difficulties experienced in working telegraph wires underground of which we have heard to-night.

The cables I refer to are made of copper wires wrapped in cotton and insulated with a compound of which resin forms a principal ingredient; they are immediately encased in molten lead by patent hydraulic machinery; the lead is put on tight, and the cable is hermetically sealed, and free from injury or deterioration by exposure to the damp atmosphere, and the insulating material is not affected by any variation in temperature. Cables can be made in any length, and covered with any weight of lead, that may be required. When intended for telephone work they are made of from 10 to 100 wires—the latter number

being enclosed in a lead covering not exceeding one inch in diameter—and are made with an internal anti-induction arrangement which I found practically did away with the annoyance caused by induction, known commonly by the name of “cross talk.” Several of these cables have now been working successfully for upwards of twelve months not only underground, but under water, and upon ordinary poles.

Mr.  
Edison.

Allusion has been made to the difficulty of maintaining a lead-covered underground cable in a satisfactory condition; but I am convinced that there is not much danger of failure in consequence of the deterioration of the lead, but rather from the decomposition or deterioration of the insulating material, caused probably by the imperfect manner in which the lead may have hitherto been applied. I have in my hand a small piece of copper telegraph wire surrounded with lead which has been in the Bramhope Tunnel, in Yorkshire, for upwards of thirty-two years; it was insulated with tar and hemp, and placed in a lead pipe. I am told that it was abandoned, after being used a few months, owing to the failure of the insulation. The lead itself, however, is perfect, the pipe being as good as on the day it was made, showing no injury from its contact with the earth. Lead water pipes are found to be in excellent condition after being in the ground for many years, and it seems only reasonable to suppose that a lead covering on a cable will have the same average life.

Mr. A. BELL: I would just remark that the essential feature in any dielectric is that it must either have flexibility or elasticity, so that in the case of wires being drawn into pipes they may be capable of being handled without any risk of the dielectric being damaged. Reference has been made to bitumen. Now bitumen has no elasticity at all—i.e., in the solid state—and it is, moreover, subject to expansion and contraction, due to the changes of temperature; and unless it were laid at a very considerable depth in the ground, where it would not be subject to such changes, it would not be reliable as an insulator.

Mr. Bell.

With reference to gutta-percha itself, I have had abundant opportunity of observing how much it decays at what are called the joint-boxes—at places where an opening is made to joint the

Mr. Bell. wires. Although great precaution and every means have been adopted to exclude the air, yet it never can be done in a perfect way; and I believe, from the appearance of the wire, that though the effect causes a special failure at the joint-boxes, the same effect takes place in a lesser degree all through the pipes, and will take place unless the pipes are buried at a great depth, and every means taken to prevent either currents of air or water percolating. In fact, I believe that many pipes act as drains, and wherever that takes place the gutta-percha is useless in a very short time; and it is difficult to prevent this unless the pipes are completely buried to a great depth, and all joint-boxes and joints carefully made. The joint of the socket pipes, which is the general mode adopted, is perhaps a little defective. I believe it would be better to turn the sockets the same as is done with water pipes, so that it could not be possible for any leakage to take place.

Mr.  
Fleetwood.

Mr. C. T. FLEETWOOD, in reply, said: Major-General Webber referred to the necessity or want of ways or tubes through the streets of London to provide the necessary wires for telephone and telegraph lines for the future. There is no doubt that the time is arriving when such ways will be necessary. For instance, at the present time, what with wire pipes, pneumatic tubes, electric light pipes, gas and water mains, and hydraulic power pipes, the whole of the space beneath the footpaths on both sides of Cheapside is fully occupied, and it will therefore be necessary, when the next wire pipe or pneumatic tube is wanted, to lay it under the asphalt in the roadway.

It has been suggested by Mr. Donald Nicoll, who has been referred to, that the curbstones should be removed from the busy thoroughfares and sold for edging suburban footpaths, replacing them with a hollow curb edged with cast steel (as on London Bridge), which should be filled with wire covered with bitumen. He states that in a curb one foot square 144 separated and insulated wires may be safely laid, and of course double that number by utilising both sides of a thoroughfare, making a total of 288 wires. We have nearly that number already in our 3-inch and 4-inch cast-iron pipes, therefore something different from

that must be provided. I would suggest that, if subways are ever adopted generally, there should be one on each side of the thoroughfare close to the houses; but it is doubtful whether house owners would care to give up the space now occupied by the vaults under the footways. Mr.  
Westwood.

Referring to Mr. Adams, I quite thought when he commenced speaking that he was going to take up the same arguments that he made use of ten years ago, instead of which he charged me with omitting to give any information as regards durability. If Mr. Adams refers to the paper, he will see that I concluded by saying that I knew of nothing to be equal to the present system for either simplicity, utility, or durability.

I have here a piece of 90-wire cable that has been down nine years, and only just recovered from along Cornhill. The gutta-percha appears to be as good as when it was put down. Here are also specimens of cables that have been down over thirty years. I was hoping that our President, Sir Charles Bright, would be present and see these old wires that were put down by him in 1852. The quality of the gutta-percha is excellent. This specimen is peculiar, as showing two portions of the cable in very different states of preservation. The difference between the portion decayed and that not decayed arises from the fact that one was drawn into the split pipes unprotected by any covering, while the other was covered with two coatings of tarred yarn. Where the coatings of tarred yarn are complete the percha is in good condition.

I referred to gas as being very troublesome. Possibly you may have heard of the explosion that took place a short time ago near Charing Cross Station. It was caused by the gas getting into the split pipes, and thus into the boxes.

In reference to mechanical joints, a number have been tried, but they have all failed. We do not suffer from our joints. Our men are all able to make what we call permanent joints; and when the material we are supplied with for making joints is good, and the covering of the wire is equal to the sheet percha, the joints turn out well. Unfortunately the covering of the wire is not always uniformly good. You have heard what has been said about

Mr. Fleetwood... compounds, mixtures, and the improvements of late years: they do not assist us in making good joints. Asphalte was also referred to by Mr. Adams. I only remember a few instances where I have seen asphalte tried. An electric light line was being put down along Pentonville Hill a few years ago, and as I watched the work I received the impression that it would not last long. I think it is there still, but the lights are not burning. Another electric light line was laid down Regent Street to Waterloo Place in the same way, and that appears to have been a failure. A few wires covered with bitumen were drawn into the iron pipes down St. James's Street, but they soon failed in insulation, and had to be drawn out and wires covered with gutta-percha substituted.

The question in reference to the underground line between Liverpool and Manchester has been answered by Mr. Preece, who has also confirmed what I have said about earthenware pipes being unsuitable for our lines, iron pipes having been substituted for those of earthenware.

The old line between the Strand and Nine Elms is well worth an examination. I should like to call attention to the fact that in 1846 the Electric Telegraph Company started the system that I still advocate—that is, solid cast-iron pipes with lead joints. If the lead joints are made well, Mr. Bell's difficulty would not be felt. I have heard Mr. Preece mention before in this room that an underground wire is only equal to one-sixteenth of an open wire.

Mr. W. H. PREECE: Commercially.

Mr. C. T. FLEETWOOD: It costs four times as much. The time will come when Mr. Preece will alter his opinion in this respect.

Mr. W. H. PREECE: I have altered it already.

Mr. C. T. FLEETWOOD: Mr. Alexander Siemens has referred to the introduction of gutta-percha in Germany, and has promised to look up the date. I fail to find any patent taken out before 1848.

Mr. Eddison mentioned lead tube cables. I do not know whether he is aware that the wire covered with lead tube that I have now here was put down about 1839, while another piece also before you was laid in 1846. I might point out that the joints in the wires were made by being twisted and soldered, and each wire

covered with a piece of tape, the whole four bound together with <sup>Mr</sup> another coating of tape; a slide, being a tube of larger gauge <sup>Fleetwood.</sup> than the piping, was then passed over, and the ends soldered down. The specimen can be examined at the close of the meeting.

As regards the durability of lead underground, I noticed in uncovering some of those old tubes after having been in the pipes forty years, that the lead has been attacked, although protected with yarn, in the iron pipes.

Upon the motion of the Chairman, the thanks of the meeting were unanimously voted to Mr. Fleetwood for his very interesting paper.

The following paper was then read :—

### THE DRIVING OF DYNAMOS WITH VERY SHORT BELTS.

By Professors W. E. AYRTON, F.R.S., and JOHN PERRY, F.R.S.,  
Members.

When fast-speed machines, such as dynamos, are driven in the usual way by belting, there is a certain waste of power in overcoming the friction that is set up between the driven spindle and its bearings by the tensions necessary to be given to the two sides of the belt to produce the required grip on the small driven pulley. This waste of power is usually kept small by using a long belt and putting the dynamo at a considerable distance from the large driving pulley, and thus obtaining a large angle of contact of the belt with the small driven pulley. But such an arrangement is very inconvenient to employ on board ship or in the guard's van of a railway train or elsewhere, when the space is confined, and a short belt would always be employed were it not that the small angle of contact of the belt with the driven pulley, that would be produced if the small pulley was put close to the large driving one, would necessitate the tension on the belt being large. And this large tension would cause considerable pressure on the bearings, leading to a considerable waste of power in overcoming friction; further, the belt would be much stretched,



requiring constant adjustment of the dynamo to tighten it to avoid slip. And further, the occasional stretching of the belt produced by atmospheric changes would make the belt slip were the tension when the belt shortens again not far greater than is necessary for driving purposes. Hence the average tension of such a belt must be made to far exceed what a simple calculation of the necessary driving tensions would give, and the waste of power is in consequence also much in excess. Indeed, this is the reason why belting is usually placed more or less horizontal, and why long belts are employed with the upper side the slack side, so as to introduce a weight compensation, as seen in Fig. 1.

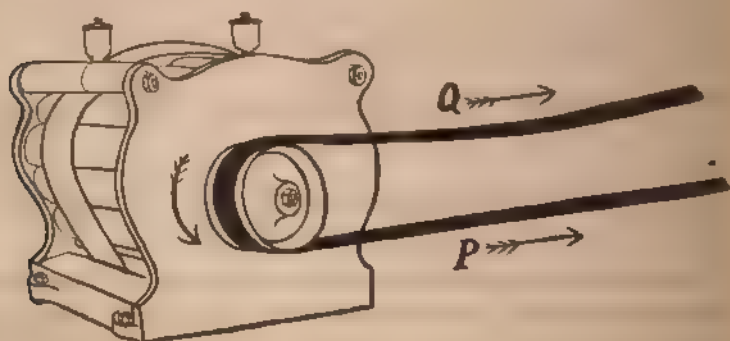


FIG. 1.

Similar difficulties occur when an electro-motor, or other fast-speed motor, is used to drive machinery by means of belting.

Very many plans have been tried of overcoming this difficulty. For example, to increase the angle of contact of the short belt with the small driven pulley, and to keep the belt tight, Messrs. Mather and Platt used a large tightening pulley with their compact engine and dynamo at the Inventions Exhibition, and also now at the Manchester Exhibition.

In transmitting power from an ordinary steam engine to a dynamo, or from an electro-motor to a tool, we have to gear a slowly and a fast rotating shaft, and the problem of doing this in a limited space and with small loss of power in overcoming friction has led to the many forms of "*friction gearing*" that have from time to time been devised. Indeed, one part of the joint work in

telpherage carried out by the late Professor Fleeming Jenkin and ourselves was the designing of many modifications of such friction gearing, or, as it has been called, "*nest gearing*," for enabling power to be transmitted between a slowly and fast rotating shaft by means of a *pure couple without side thrust*. To enter into even a short description of these many forms would take up too much of your time; but we may mention that those interested in the subject will find descriptions of some of them in papers read before the Society of Arts and the British Association. The great objection to the employment of such friction gearing is the expense of construction, since adjustments have to be introduced to take up the wear of the rolling pulleys, which is considerable; hence, in spite of the ingenuity of friction gearing, from a mechanical point of view, and of the captivation it has for those who are concerned with this problem of transmitting power between slowly and fast rotating shafts, it has not come into general use.

You are probably familiar with the form of friction gearing employed by Messrs. Siemens\* for driving dynamos. The dynamo is balanced so as to turn on an axis at right angles to the axis of rotation of the armature and the dynamo pulley; the pulley is made of compressed paper, and rests on the top of the fly-wheel of the engine. The arrangement is good, since the force pressing the pulley against the rim of the fly-wheel is not all transmitted through the bearings, but is partly furnished by the weight of the dynamo pulley. But this form of friction gearing has the following objections. First, the paper pulley has to be constructed in a very special way, under hydraulic pressure, and is necessarily much more expensive than an ordinary cast-iron pulley. Secondly, the fly-wheel of an ordinary engine cannot be employed without having first its rim turned up true. Thirdly, the axes of rotation of the armature and fly-wheel must be placed accurately parallel to one another, otherwise there is a grinding action at the surface of the paper wheel, which destroys its surface. Next, it is a mistake to place the dynamo so that the paper pulley

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\* Since the writing of this paper we have learnt that this ingenious method of driving is due to Mr. Raworth.

rests on the very top of the fly-wheel, since with this arrangement there is necessarily a side thrust on the bearings, and the power wasted in friction at the bearings cannot be made as small as is possible. And lastly, we are told by users of this form of gearing, that there is a tendency for the whole framework supporting the dynamo to get into a state of vibration. The result is that this method is not one by means of which any dynamo can be arranged so as to be driven directly by the fly-wheel of any engine, and is only employed when the engine and dynamo are constructed so as to form practically one piece of machinery.

A form of gearing employed for driving a dynamo with a gas engine may be seen in the City, and at first sight this might appear to be merely the inversion of the gearing just described, because in this latter the pulley of the dynamo, instead of resting on the top of the engine fly-wheel, is pressed up against it below. But although this makes a less cumbersome arrangement, there is not any saving in the power wasted in friction at the bearings of the armature spindle, as there is in the Siemens gearing, since with the latter arrangement a force equal to the sum of the weight of the pulley and the force pressing the pulley up against the fly-wheel has to be transmitted through the bearings. It is not, therefore, necessary to consider this form of gearing in detail, since it is not one that saves any portion of the waste of power in the friction at the bearings.

We now come to the new method which we wish to bring before you, and which consists in hanging the dynamo pulley,  $L$ , from a short belt passing round the engine fly-wheel,  $F$ , as seen in Fig. 2, the belt being only just long enough to embrace the fly-wheel and dynamo pulley without the two being brought into contact. The dynamo,  $D$ , as in the previous case, is supported so as to turn round an axis at right angles to the axis of rotation of the armature, and the method of fixing the dynamo is as follows:—The pulley is removed from the spindle, and the dynamo is fixed in a cradle,  $CO$ , turning on trunions,  $TT$ , in such a position that the dynamo and cradle just balance. Then, if  $p_1 p_2$  be the pressures of the armature spindle on the two bearings,  $B_1 B_2$ , respectively nearer to and further from the pulley end of the dynamo.

$p_1 + p_2$  = weight of armature, commutator, and spindle.

The pulley, which is heavier than an ordinary dynamo pulley, but, like an ordinary pulley, simply made of cast iron, is now keyed on to the armature spindle and hung in the belt, as seen in the

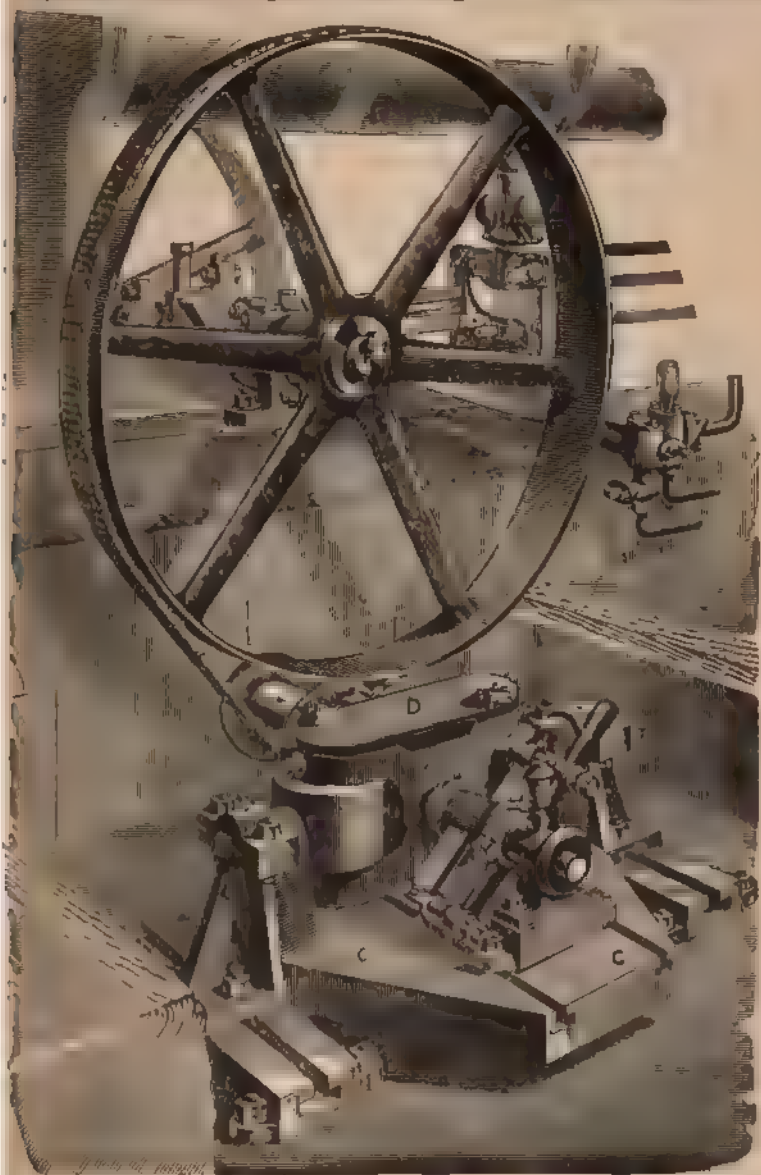


FIG. 2. FLOOR SEEN CUT AWAY IN FRONT TO CLEARLY SHOW CHADLE SUPPORTING DYNAMO.

Fig. 24, and as the entire weight of the pulley is supported by the belt, the dynamo by itself still balances, and the pressures on the bearings are simply  $p_1$  and  $p_2$  as before. The pulley,  $L$ , is made of cast-iron weight, and the crane carrying the dynamo is placed in such a position relatively to the fly-wheel, that the resultant,  $R$ , of the tensions for the two sides of the belt, when the dynamo is receiving the maximum driving power, is equal to the weight of the pulley, and acts vertically through its centre, together with the necessary driving couple,  $VV$ , as seen in Fig. 3. The horizontal components of the tensions  $P$  and  $Q$  in the two sides

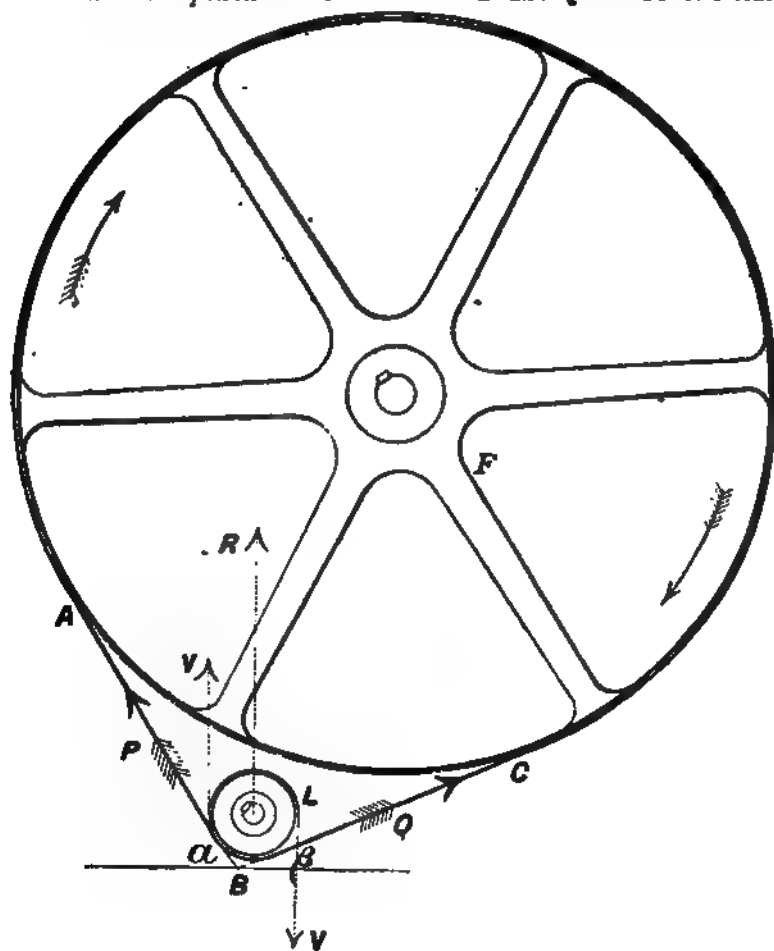


FIG. 3.

of the belt are in equilibrium by themselves, so that the pressures exerted by the spindle of the armature on its bearings are simply equal to the weight of the armature, commutator, and spindle that is, are even less when the dynamo is receiving the maximum driving power than in the case of an ordinary dynamo when the armature is *at rest*, since our form of belt gearing, unlike any ordinary belt, communicates *simply a pure couple* to the armature, instead of a couple together with a smaller or greater thrust of the spindle against the bearings.

And not only with this mode of driving need no extra pressure be thrown on to the bearings, but the pressure that is produced merely by the weight of the armature can be diminished. In fact the pressure on the bearing,  $B_1$ , nearer to the driving pulley *can be made actually equal to nought* by slightly altering the adjustments in the following way :—Instead of fixing the dynamo in the cradle so that it balances when the driving pulley is removed, fix the dynamo a little nearer to the fly-wheel, so that the pulley end of the dynamo is a little the heavier, and move the cradle,  $C C$ , a corresponding distance away from the fly-wheel, so that the pulley, when keyed on to the armature spindle, and the fly-wheel are properly in line. Then the tensions in the two sides of the belt are balanced, not merely by the weight of the pulley, but by this weight plus any portion, or the whole, of  $p_1$  depending on the exact position in which the dynamo has been fixed on the cradle. Hence the weight of the pulley may be made smaller by an amount equal to the portion of the pressure,  $p_1$ , which is carried by the belt, and the pressure on the bearing,  $B_1$ , nearer the pulley, which, with an ordinary dynamo, is by far the larger, can, with this arrangement, be made as small as we like, or even nought, whether the armature be at rest, or the dynamo be producing its maximum power. The pressure on the other bearing,  $B_2$ , will of course never exceed  $p_1$ , which is usually equal to about half the weight of the armature, commutator, and spindle. And, since the diameter of the spindle on the side farther away from the pulley can be made comparatively small, as power is not transmitted through it, the power spent in overcoming friction at this bearing,  $B_2$ , will be trifling. Hence the total waste of power at

the bearings can be made practically nought by employing this form of belt gearing.

And further, this gearing has the following advantages not possessed by the Siemens gearing :—

1. There is no paper pulley to be constructed and to wear out.
2. The rim of the engine fly-wheel need not be specially turned up.
3. The axes of rotation of the armature and fly-wheel need not be accurately parallel, as the yielding of the belt allows for want of perfect parallelism.
4. The cradle of the dynamo is low down, and therefore can be made steady.

It is also clear that any slight stretching of the belt will not cause it to become slack, since the only effect will be to lower the pulley slightly, and incline the axis of the armature to the horizontal.

To further illustrate this method of belt gearing, we give the actual calculations made for determining the proper weight to give to the dynamo pulley, and the proper position in which to place the dynamo, which is now being driven by a short belt at the Central Institution.

The four things to be determined are—

1. Proper Size of Dynamo Pulley.
2. Proper Weight of „ „
3. Proper distance to place Axis of Armature below Axis of Fly-wheel.
4. Proper distance to place Axis of Armature to one side of Axis of Fly-wheel.

1. The dynamo in question requires about 6 horse-power when running at 1,600 revolutions per minute, and when giving out the safe maximum number of watts. The fly-wheel of the engine is 4 feet 11 inches in diameter, and runs at 180 revolutions per minute; therefore, in order that the dynamo may be driven by a belt directly from the fly-wheel, the diameter of the dynamo pulley must be

$$\frac{180}{1,600} \times 4' 11'', \text{ or } 6\frac{1}{2} \text{ inches.}$$



2. Let  $P$  and  $Q$  be the tensions, in pounds, of the belt on the tight and slack sides respectively, when the dynamo is receiving 6 horse-power, then

$$(P - Q) \times 1,600 \times \pi \times \frac{53}{8 \times 12} = 6 \times 33,000,$$

$$\text{or } P - Q = 71.3, \text{ say } 72, \text{ lbs.}$$

If the rim of the dynamo pulley were in actual contact with the rim of the fly-wheel, it is easy to show that the angle between the belt on the two sides of the pulley would be  $105^\circ$ . The pulley and fly-wheel must not touch, since they would be moving in opposite directions at the place of contact, but, in order to make the arrangement compact, it is desirable that the pulley should be as near the fly-wheel as it conveniently can be without touching, therefore let us take  $100^\circ$  as the angle that the belt shall make on the two sides of the pulley. Then, since the belt makes a tangent with the pulley, the arc of contact of the belt with the rim of the pulley must be  $180^\circ - 100^\circ$ , that is  $80^\circ$ , or  $\frac{4}{9}\pi$ . Therefore, if  $P$  be the tension of the tight side of the belt,  $A B$ , and  $Q$  that of the slack side,  $B C$ ,

$$\frac{P}{Q} = e^{\mu \times \frac{4}{9}\pi}$$

$$\therefore P = (P - Q) \frac{e^{0.1773\pi}}{e^{0.1773\pi} - 1},$$

and, as just shown,  $P - Q$  equals 72 lbs.

$$\text{Therefore } P = 169 \text{ lbs.,}$$

$$\text{and } Q = 97 \text{ lbs.}$$

By the triangle of forces it is seen that the resultant of  $P$  and  $Q$  equals 180 lbs. Therefore if the dynamo is so placed that half the weight of the armature and spindle, which is  $\frac{94}{2}$  lbs., is borne by the belt as well as the pulley itself, the weight of the pulley must equal

$$180 - \frac{94}{2}, \text{ or } 133 \text{ lbs.,}$$

and a pulley of this weight was cast, and is at present in use on the dynamo at the Central Institution.

3 and 4. To determine the position in which the axis of the

armature should be placed below and to one side of the axis of rotation of the fly-wheel, we have the condition that the horizontal components of the tensions on the two sides of the belt should be equal and opposite in order that there may be no side thrust. Hence, if  $\alpha$  and  $\beta$  be the angles that the two sides of the belt make with the horizontal (Fig. 3),

$$\begin{aligned} P \cos. \alpha &= Q \cos. \beta, \\ \text{or } \frac{\cos. \alpha}{\cos. \beta} &= \frac{Q}{P} \\ &= 0.574. \end{aligned}$$

And since the angle between the two sides of the belt is  $100^\circ$ ,

$$\begin{aligned} \therefore \alpha + \beta + 100^\circ &= 180^\circ, \\ \text{or } \beta &= 80^\circ - \alpha, \\ \therefore \cos. \alpha &= 0.574 \cos. (80^\circ - \alpha), \end{aligned}$$

and expanding, we have

$$\begin{aligned} \tan. \alpha &= \frac{1 - 0.574 \cos. 80^\circ}{0.574 \sin. 80^\circ} \\ &= 1.59, \\ \therefore \alpha &= 57^\circ 50', \\ \text{and } \beta &= 22^\circ 10', \end{aligned}$$

Plotting these results to scale, it is found that the distance of the centre of the dynamo pulley below the centre of the fly-wheel must be 2 feet  $8\frac{1}{2}$  inches, and the distance sideways  $10\frac{1}{2}$  inches, a result which is also confirmed by calculation. And this is the position in which the dynamo was placed some six months ago.

As a proof of our conservative familiarity with the ordinary method of driving a dynamo, instead of with the principles of belt gearing, the first remark made by almost every one who has seen the new gearing is, that the belt must slip very much. As a matter of fact, however, so far from there being any serious slip, experiment shows that the dynamo, when giving out its maximum power, rotates a little faster than the calculation makes it, due of course to the diameters of the fly-wheel and pulley not being exactly as stated in the calculation.

We need not dwell here on the fact that ropes may be used instead of belts. Again, when a number of dynamos have to be driven from one fly-wheel in a confined space, one driving belt

may be used for all, and, by means of guide pulleys, arranged so that each dynamo pulley lies on the belt between two guide pulleys, the part of the belt on one side of each dynamo pulley being nearly horizontal and on the other side nearly vertical.

Mr. ALEXANDER SIEMENS: I do not want to disclaim in any hostile spirit the friction gear which Professor Ayrton has just now condemned, which, although called "Siemens," was originally designed by Mr. Raworth, who was at the time acting as our agent. In reply to Professor Ayrton's criticisms, I would say that by balancing the cradle and dynamo without the paper pulley you can get the bearing entirely free from any pressure due to gravity, just as he described in the case of his own. Then, although in some of the small dynamos the weight of the paper pulley is quite sufficient to produce the necessary friction, in most cases rods and springs are provided to increase the friction. In judging of gear like this it should be borne in mind what it has been designed for. Professor Ayrton has spoken of the difficulties in getting paper pulleys, but they can be obtained as easily as ordinary pulleys, and to get a wheel turned flat for any particular purpose you have simply to give the order to the maker and he will do it. I may tell you that we have supplied one single company with 20 sets of this friction driving gear; we have it adapted for from 4 to 40 horse-power, and everybody likes it, especially so because the engine which is driving the arrangement can be of the ordinary type.

There is no doubt that the most desirable way of driving dynamo machines is by direct coupling, only then you require such a complicated steam engine that a marine engineer does not like it, and therefore Mr. Raworth designed the friction gear so that it could be used on board ship and with a steam engine of one of the various forms of simple construction.

Mr. J. S. RAWORTH: As being the original designer of the form of friction gear, I have just one or two words to say. Mr. Alexander Siemens is wrong in supposing that it was gathered in any sense from "nest gearing," because I think he will remember, on going back to the time, that it was anterior to the advent of any form of nest gearing.

Mr. ALEXANDER SIEMENS: I simply wanted to remind you.

Mr.  
Raworth.

Mr. RAWORTH: Exactly. The principal point that strikes me about the form of gearing before us is that it does not quite come up to the description that has been given of it as being applicable to any form of steam engine. Professor Ayrton has not shown us on his drawing the steam engine which is supposed to be driving the fly-wheel; but it appears to me that it must either be a horizontal engine packed up with about five feet of brickwork, or it must be a vertical engine with the cylinder above the diagram, or it must be one of the old-fashioned vertical engines with a cylinder down on the floor and the crank above. Now we all know very well that that form of engine has gone completely out of use, and one cannot be obtained now, except specially; and when you get it, it is a very much more expensive form of engine than the ordinary inverted engine with cylinders on the top, because large framing has to be employed to make the crank shaft anything like steady when working at our usual high speeds. Mr. A. Siemens has said that the question of the price of the pulleys in friction gear is not serious, because in Lancashire you can buy paper pulleys almost as readily as cast-iron pulleys. But when you come to the question of cost you have to consider that the cast-iron pulley shown in the diagram is a solid pulley which would have to be cast with a large head of metal, making the weight perhaps twice as much as that of the finished article; and therefore, when this is taken into account, I think it would cost just about as much as the paper pulley; and when you come to the question of steadiness it is very much the same as in the friction gear, and, if one is liable to vibration, the other would be. In the early days, of course, some dynamos vibrated a little, but the cause of that was that they were not so well balanced as now, and I know plenty of cases where dynamos are running perfectly well without vibration and at considerable speeds without any difficulty.

I would point out for the benefit of members of the Society that the chalk diagram [*shown on board by Mr. A. Siemens*] showing my arrangement of friction gear is not exactly correct. The shaft which is on the left-hand side should really extend on

the right-hand side, underneath the dynamo, and the off end of the shaft should be shown on the left-hand side. It will now be seen that the engine and dynamo, as far as ground space go, occupy pretty much the same room, and that you do not have a separate space for the engine and a separate space for the dynamo, but that the two stand nearly upon the same base. Of course the plan that Professor Ayrton has shown us is very interesting, but we cannot thoroughly understand its relative merits without having the engine shown on the same plan, that we may see what space the two take up together.

I would call your attention to a suggestion at the end of the paper that a number of dynamos might be put in a row and driven by one strap going over a guide pulley, down on the dynamo pulley, and up over another guide pulley, down on another dynamo pulley, and so on, to a considerable number in series. Now I think it is obvious that if that were done, and the power put on, the first dynamo would tip up on end, the second dynamo would assume an inclined position, and only the last dynamo of all would work under the conditions that Professor Ayrton has designed to be correct.

Perhaps my meaning would be clearer if I said that the strap in passing over the pulley of the first dynamo should have enough pull to drive all three or four dynamos in succession; that on the second dynamo pulley the pull on the strap would be reduced by, say, one-fourth (if there were four dynamos); on the third pulley it would be reduced to one-half; and on the last pulley it would only have the pull required to drive one dynamo; and therefore in this series arrangement the moment the power was put on the first dynamo would sit up.

Mr. GISEBERT KAPP: The paper is a very remarkable one, Mr. Kapp because it seems to contradict our old notions about belt friction. The authors have assumed in their calculation a coefficient of  $\cdot 4$  for the friction between the belt and the pulley. This is a larger coefficient than usually assumed. I have looked up the figures in various works, and find the following:—Rankine gives  $\cdot 235$ ; Wiesbach,  $\cdot 28$ ; a German technical book called "Hutte," which is a standard work, gives  $\cdot 29$ . Practical engineers are in the habit of



ten o'clock, to make a working drawing, and my sketch must be understood as being merely suggestive of the Raworth gearing. Professor  
Ayrton.

I do not follow Mr. Siemens in seeing how it is possible to have no objectionable side thrust if the paper pulley be put on the top of the fly-wheel, as I have always seen it done with the Siemens, or, rather, the Raworth gearing. It ought to be put slightly on one side of the fly-wheel, and not on the top. The initial cost of a paper pulley may not be so very much greater than that of a cast-iron pulley such as we employ; but the cost of maintenance, which is rather what we refer to in our paper, is, as far as my experience goes, certainly far greater for paper pulleys than for iron ones. Mr. Siemens says that you can order the fly-wheel of an engine to be turned true when the engine is being made. That is so, but if you have to deal with an ordinary engine in position it is by no means such an easy matter to get the fly-wheel trued up. It would have to be taken off the engine and sent to the manufactory, and even then, as it would not have been turned upon the engine shaft, it would probably be found to be out of truth when keyed on subsequently.

The engine that is being employed to drive the dynamos at the Central Institution is not a special nor an antiquated form, as Mr. Raworth seems to think, nor is it raised on eight feet of brickwork. It is one of Marshall's semi-portable compound engines, and was bought second-hand at the sale of the stock of the Hammond Company, in whose factory it had been in regular use for driving dynamos. The engine is fitted with two fly-wheels, one on each side, one being employed at the Central Institution to drive coned shafting. From this shafting several dynamos are driven, and the speed of any one of them can be varied between wide limits, without affecting the speeds of the others. From the other fly-wheel hangs the balanced dynamo, as shown in Fig. 2; and the engine is not mounted on eight feet of brickwork, as suggested by Mr. Raworth, but the dynamo is placed in a shallow hole dug out of the floor to receive it with its cradle on which it rests. In the figure 2 the hole in the floor is intentionally shown much larger than it really is, in order that the cradle supporting the dynamo may be clearly seen. I



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...that we are justified in saying that our form of gearing is superior to any ordinary form of steam engine; whereas the Raworth gearing is only employed when the engine and pulley are constructed so as to form practically only one piece of machinery. My reason for considering that our gearing can be made more free from vibration than the Raworth's is that with ours the dynamo and cradle are set down, while with his they are mounted several feet up in the air.

As to Mr. Raworth's criticism that if many dynamos were driven with one belt or rope, as suggested at the end of our paper, the dynamo would tip up, I merely reply that it is a simple question of calculation to adjust the angles of the belts and the weights of the pulleys so that the tensions in the various parts of the belt shall merely produce the necessary driving couples and a set of forces respectively equal and opposite to the weights of the various dynamo pulleys or to the weights of these pulleys plus any portions of the weights of the armatures, which, as explained in our paper, may be carried also by the belt. In fact, if the method of calculation described in our paper be followed, and the various parts of the belt be made to have the angles given by the calculation, and also the pulleys their proper weights, there will be no tipping action such as Mr. Raworth fears.

Next, in reply to Mr. Kapp: I do not at all follow him when he says that Rankine gives 0.235 for the coefficient of friction. Once we find in Rankine 0.56 for leather on dry metals, and 0.36 for leather on wet metals. Further, Rankine says that for the general driving of metal pulleys by leather belts it is quite safe to take 0.42 as the coefficient of friction. Now this is actually a higher number, and not a lower one, than the coefficient of friction we have employed in our calculation. Mr. Kapp's calculation as to the necessary weight to give to the pulley I therefore do not assent to. But even if he were correct in his value of the coefficient of friction, and even if it were necessary to have such a heavy pulley as he makes out, there would not be the disadvantage that he imagines, since a heavy pulley and a heavy fly-wheel may be made in very different ways. A pulley may be made heavy by putting the mass near the axis; whereas a

fly-wheel, to have a considerable moment of inertia, must have its mass far from its axis of rotation, and it is only then that inequality in the density of the metal becomes serious. Then bear in mind the weight of the heavy pulley is not borne by the bearings of the dynamo; the pulley simply rests in the belt. In fact, the heavier it be necessary to make the pulley for efficient driving, the more valuable is our arrangement, since the weight of our pulley, which is carried entirely by the belt, and which therefore exerts no pressure on the bearings of the dynamo, is under ordinary circumstances replaced by the pressure exerted through the bearings. No greater pressure on the belt is required by our method of driving than by any other with the dynamo equally near the fly-wheel; the only difference is that in our case we produce the pressure on the belt *solely by gravity*, where as usually it is *wholly* transmitted through the bearings of the dynamo, producing a corresponding amount of waste of power in friction.

The CHAIRMAN: Gentlemen,—This is the last meeting before the recess, and I think you will agree with me that the session has so far been a successful one, for we have had important papers on a variety of subjects, and several of them have led to discussions of full length; on the whole, I think we have had an exceedingly interesting and successful session.

A ballot took place, at which the following were elected:—

*Member:*

Musgrave Heaphy.

*Associates:*

Ernest L. Berry.

James Davey.

Alexander Hill.

J. E. Kingsbury.

Walter Palmer.

*Students:*

Alfred H. Dykes.

Walter Chas. Garrard.

Charles G. Lamb.

John Henry Tonge.

Chas. Henry Yeaman.

The meeting then adjourned until November 10th.

# THE LIBRARY.

## ACCESSIONS TO THE LIBRARY FROM MARCH 31 TO JUNE 15, 1887.

(Works marked thus (\*), have been purchased. Of those not purchased or received in exchange, where the donors' names are not given, the works have been presented by the authors.)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD PRESENT COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

**Abel** [Sir Frederick], C.B., F.R.S. The Work of the Imperial Institute. Address delivered at the Royal Institution of Great Britain before H.R.H. the Prince of Wales, K.G., F.R.S., April 22nd, 1887. 8vo. 33 pp. London, 1887

**Ayrton** [W. R.], F.R.S. Practical Electricity: A Laboratory and Lecture Course for First Year Students of Electrical Engineering, based on the Practical Definitions of the Electrical Units. 8vo. 516 pp. London, 1887

**Bateman-Champain** [Col. Sir J. U.] [Vide Smith.]

**British Guiana.** Balata and the Balata Industry, Forest Laws, &c. Report by G. S. Jenman. Fo. 38 pp. Demerara, 1885  
[Presented by the Colonial Office.]

**Chappe** [M. L'Ainé]. Histoire de la Télégraphie. 8vo. 267 pp. Paris, 1824  
[Presented by Prof. Silvanus Thompson, Member.]

**Dolbear** [Amos E.] et al v. American Bell Telephone Co., &c. The Telephone Appeals. Argument of E. N. Dickerson for the American Bell Telephone Co. 8vo. 160 pp. New York, 1887  
[Presented by American Bell Telephone Co.]

**Ewing** [Prof. J. A.], B.Sc., F.R.S.E. Effects of Stress and Magnetisation on the Thermo-electric Quality of Iron. 4to. 21 pp. Plates. [Phil. Trans., Part II., 1886, p. 361.] London, 1886

**Gee** [W. W. Haldane], B.Sc. [Vide Stewart and Gee.]

\* **Hering** [Carl]. Practical Directions for Winding Magnets for Dynamos. 12mo. 63 pp.

**Imperial Institute.** [Vide Abel, Sir Frederick.]

**Jenman** [G. S.] [Vide British Guiana.]

**Light** [Charles J.] [Vide Society of Engineers.]

**Martin** [Thomas Commerford] and **Wetzler** [Joseph]. The Electric Motor and its Applications. 8m. fo. 208 pp. New York, 1887

**Royal Engineers' Institute.** Occasional Papers. Vol. XI., 1885. Professional Papers of the Corps of Royal Engineers. Edited by Capt. Francis J. Day, R.E. 8vo. 266 pp.

Chatham, 1887

[By Exchange.]

**Royal Institution of Great Britain.** Proceedings. Vol. XI., Part III., No. 80. 8vo. 265 + viii. pp. [335 to 599 pp.]

London, 1887

[By Exchange.]

**Smith** [Col. B. Murdoch], R.E. In Memoriam—Col. Sir John Bateman-Champain, R.E., K.C.M.G. A Short Sketch of his Career. 12mo. 18 pp.

London, 1887

**Smithsonian Institution.** Annual Report of the Board of Regents, showing the Operations, Expenditures, and Condition of the Institution for the Year 1884. Part II. 8vo. 468 pp.

Washington, 1885

[By Exchange.]

**Society of Engineers.** Transactions for 1886, and General Index, 1861-85. Edited by Charles J. Light, Secretary. 8vo. 269 pp.

London, 1887

[By Exchange.]

**Stewart** [Balfour], M.A., LL.D., F.R.S., and **Gee** [W. W. Haldane], B.Sc. Lessons in Elementary Practical Physics. Vol. II.—Electricity and Magnetism. 12mo. 497 pp.

London, 1887

[Presented by Messrs. Macmillan & Co., Publishers.]

**United States Ordnance Department.** Annual Report of the Chief of Ordnance [Brig.-Gen. S. V. Benét] to the Secretary of War for the Fiscal Year ended June 30, 1886. 8vo. 533 pp.

Washington, 1886

[By Exchange.]

**Viale** [G.] Istruzioni sul Sistema Automatico Wheatstone. 8vo. 117 pp.

Roma, 1887

[Presented by the Commander F. Salvatori, Foreign Member.]

**Wetzler** [J.] [Vide Martin and Wetzler.]

**Wheatstone.** [Vide Viale.]

## ORIGINAL COMMUNICATIONS.

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### THE RESISTANCE OF FAULTS IN SUBMARINE CABLES.

The following communication has been received from Mr. A. E. Kennelly in reply to some of the remarks in the discussion on his paper read on the 24th March, when he was absent from England.

It can hardly be doubted that the apparent resistance of an exposed end buried in mud, differs from that which it would offer, *ceteris paribus*, if freely exposed in sea water. It may, perhaps, be safe to say that, generally speaking, buried ends offer higher and more variable resistances to tests with the zinc pole to line, and lower steadier resistances with the copper pole to line, than is ordinarily met with. This is easily accounted for by supposing that with zinc to line the hydrogen evolved on the exposure, being more or less completely imprisoned, tends to seal up the fault, while with copper to line, salts of copper only being formed, the closer connection of the conductor with the earth more readily reveals itself. It may be mentioned that out of thirteen different series recorded since February, 1886, on different broken cables—six of which appear in the paper—only one series failed to give a good and useful set of readings. In this particular case, the resistances of all currents with the zinc pole to line were too variable to observe, and did not seem to follow the law of inverse square roots of currents; but with copper to line a series of very steady readings, with currents from 1 to 25 milliampères, was obtained, in which the resistance of the fault followed the law not accurately but fairly. This broken end not being recovered in the repair which followed, it is impossible to speak with certainty, but it is probable that the fault at the time of testing was imbedded in mud; and a similar failure of a series with zinc to line in future would be open to that interpretation if to no other.

In laboratory experiments on the resistance of exposures,

results have at times been arrived at which are not in accordance with the laws of inverse square roots. Two possible sources of error may be pointed out.

First.—If the method of bridge measurement described in the paper be followed, and an ordinary testing key be employed to throw the testing battery in and out of its circuit between bridge and earth, then, since theoretically the total resistance of the circuit should be constant in all positions of the key, the disconnection during the key's play is liable to affect the "immediate-false-zero." It is, therefore, advisable to use a battery of such strength that the resistance  $\rho$  inserted to adjust the current may be high relatively to the resistances in the bridge.

Second.—The exposure experimented on should have its surface so disposed that the gas evolved may discharge from it freely and not lodge in crevices or beneath ledges. For example, a small exposure imbedded in insulating material, or a small cable core exposure pointing vertically downwards, will not generally yield the laws. For gas will accumulate beneath the insulating ledges more rapidly and fully with the stronger than with the weaker currents. Consequently, the stronger currents will encounter higher resistances by amounts due to the relative reduction of the area of liquid contact, and in the equation  $R = k C^{-\frac{2}{n}}$ , where  $R$  is the resistance observed,  $C$  the current sent, and  $k$  a constant,  $n$  will tend to appear greater than 2.

GIBRALTAR, 7th May, 1887.

## ON THE MEANS EMPLOYED TO DEVELOPE FACTORY FAULTS IN SUBMARINE CABLES DURING MANUFACTURE.

By CHARLES BRIGHT, Jun., Member.

On hearing Mr. A. E. Kennelly's paper, on the "Resistance of Faults in Submarine Cables," it occurred to me that a few notes on the means employed to bring about an early development of

"factory" faults during a cable's manufacture might be of some interest in connection therewith.

In the first cables, previous to 1860, the hemp or jute serving in which the core is embedded was invariably saturated with tar as a preservative against decay by the sea-water.

In 1860, however, Mr. Willoughby Smith first pointed out that the tar as being an *insulating* fluid had a tendency to "mask," or even seal up temporarily, any small faults that might exist in the gutta-percha envelope. The insulating properties of tar being of a low, unreliable, and temporary nature, the above effectual concealment (beyond the search of an electrical test) would last until the faults were subjected to any strain (as by submergence) or to the searching conducting properties of the sea, when they would suddenly break out at a somewhat inconvenient stage, as was, no doubt, hitherto frequently the case.

We are next indebted to Mr. Willoughby Smith for then introducing the application of tannin (the bitter element of bark, a preservative to any fibrous material), or of brine, in place of the tar, as being a *conducting* rather than an insulating fluid, and thus possessing the tendency to "show up" or even develop any such faults, on the application of the jute round the core, instead of temporarily concealing them, as the tar tended to.

This method has since been very generally adopted, the jute and hemp servings being almost universally steeped in tannin (or "cutch," as it is often termed) previous to use in submarine cable construction. Sometimes the jute bedding, in which the core is enclosed, is applied *wet*, so that the object aimed at is more readily attained on the compression of the iron wires from the lay-plate during the process of sheathing, which has the effect of *squeezing* the water (or "cutch") into any little aperture or mechanical defect that may exist in the gutta-percha covering. By this practice, then, in conjunction with a continuous electrical test for insulation being kept on the core whilst it is being covered, a "factory" fault may often be detected on the instant of application of serving or sheathing wires over the defective part, and a repair very readily carried out; in striking contrast to the great amount of trouble (involving proportional expense)



that such a fault might have led to afterwards, if not developed until the course of, or still more after, submergence. This should not be forgotten as one of the principal advances made just at that very critical period of submarine telegraphy, when so many of the improvements now in practice were first introduced.

In 1858, a patent (No. 1,811) was taken out by Mr. Willoughby Smith for an adhesive composition, which is now almost universally employed in all submarine cables, as being an excellent medium for cementing together the separate coatings of gutta-percha (where it is laid on in more than one coating), besides sealing the first coat with the copper conductor. This compound (which is perhaps somewhat erroneously known as "*Chatterton's* "compound") is usually composed of the following proportions by weight—viz., gutta-percha, 3; resin, 1; and Stockholm tar, 1. As was implied by the President, in opening the discussion on Mr. Kennelly's interesting paper, it is not improbable that small faults, or deficient insulation, in any one of the coats, may, inadvertently, be temporarily "masked" by even *this* small proportion of tar in the above adhesive composition applied between each covering.

Whether or no this possibility is sufficient to warrant the necessity, or even the advisability, of an insulation test being taken on the application of *each* coat of G.P. (*previous* to the adhesive compound being applied ready for the next "coat" or, perhaps, even afterwards as well), is a point of some interest and importance, only to be determined by those responsible and most experienced.

The only tar outside the *outer* coat of the completed core in a submarine cable of to-day is that in the Bright and Clark's bituminous compound as applied round the iron sheathing wires, but the "tanned," or wet, jute immediately surrounding the core should have sufficient time to "bring out" any "factory" fault before the composition has found its way between the interstices of the wires.

In conclusion, it is perhaps scarcely necessary to mention that "factory" faults are the only class of faults over which we can exercise any control in the sense of producing an early

development at a convenient stage for remedy; but perhaps it would not be too much to say that it is quite possible that more faults than it is often at present supposed, are really originally due to the "factory" during a cable's construction—faults which have not developed themselves until some time afterwards, and are taken to be due to local causes at the time of "breaking out."

This would point to the positive necessity of every possible effort being made to discover in submarine cables, during their manufacture, faults which, though perhaps very minute at first, may afterwards be the cause of much trouble and expense.

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## ABSTRACTS.

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### E. BLONDLOT—RESEARCHES ON THE TRANSMISSION OF ELECTRICITY OF LOW TENSION THROUGH HOT AIR.

*Journal de Physique*, Vol. 6, p. 109, 1887; *Bulletin de la Société Internationale des  
Electriciens*, Vol. 4, p. 130, 1887.)

The author recalls a work published by him in 1853 in the *Annales de Chimie et de Physique*, which confirmed the discovery of Mr. Becquerel that gases brought to high temperatures allow a current of electricity to pass, even when it is produced by a single cell.

He arranged two perfectly insulated electrodes, formed by two discs of platinum 3 centimetres in diameter, in the upper part of a porcelain crucible entirely closed at the top and open below. This porcelain crucible was itself placed inside an iron cylinder, also closed at the top, and placed in a gas furnace. The two platinum discs were mounted on insulating rods which descended vertically, and were then bent horizontally, so that the insulating clamp which held them was quite removed from the action of the furnace.

The arrangement being heated above, the author found, that it is only on attaining a red heat, that a capillary electrometer shows the passage of a current through the heated air. However, in previous experiments he has found that the column of hot air which rises from an incandescent body allows of the passage of the current from a single cell, when a thermometer placed in it shows a temperature of only 70° C. In the author's opinion this is due to the high temperature of some streams of air which alone conduct the current. The experiments prove that the current passes with any difference of potential in some instances less than one-thousandth of a volt.

The chief object of the article is to determine if the laws of the transmission of electricity through a heated gas are the same as those which govern its transmission through solid and liquid bodies. Mr. Becquerel had noticed some contradictions, specially that the resistance seemed to depend on the intensity of the current, and on the number of cells used. Mr. Blondlot has noticed that the flow of electricity increases more rapidly than the difference of potential: he concludes from this that, if the resistance of heated air is calculated by the methods applicable to solids and liquids, a number is arrived at, which is dependent on the E.M.F. and on the current; but this method ought not to be pursued, the mode of action of this transmission being apparently Faraday's convection—that is to say, the carrying of the electricity by molecules of air, which come and charge themselves on each electrode, then move towards the opposite ones in consequence of electric attractions and repulsions, and discharge themselves there. This convection is impossible when

the air is cold, on account of the adhesion of the molecules of air to the surface of the platinum. Whether this hypothesis be true or not, the value of the experimental results is not affected thereby.

### E. BUDDE—ELECTRO-DYNAMIC LAWS.

*Bulletin de la Société Internationale des Electriciens*, Vol. 4, p. 183, 1887, *Annalen der Physik und Chemie*, Vol. 29, p. 488, 1886, and Vol. 30, p. 100, 1887.)

The author proposes a series of experiments by the help of which a decision may be arrived at, as to which electro-dynamic theory best represents the facts of electro-dynamics.

Weber considers the elementary action of two electrified bodies  $e$  and  $e'$ , at a distance  $r$  apart, and expresses it by the formula

$$Q = \frac{ee'}{r^2} \left\{ 1 - \frac{1}{2C^2} \left( \frac{dr}{dt} \right)^2 + \frac{1}{C^2} \left( \frac{d^2r}{dt^2} \right) \right\},$$

in which  $C$  is a constant of the magnitude of the velocity of light.

According to Riemann, the potential of two electrified bodies is defined by the equation

$$V = ee' \frac{r^2}{C^2} \cdot \frac{d}{dt} \cdot \frac{1}{r} \cdot \frac{d}{dt};$$

and according to Clausius the value of the potential is given by the equation

$$V = \frac{2}{C^2} \frac{ee'}{r^2} v v' \cos. \theta,$$

where  $v$   $v'$  are the velocities of the two electrified particles, and  $\theta$  the angle of these two velocities.

The author has calculated for each of these theories what would be the results of certain experiments particularly suitable for their verification.

1. Charge and discharge of a hollow metallic sphere, inside which a magnet is suspended by a cocoon fibre so that its magnetic axis is vertical. This magnet would be unaffected, according to Clausius; would experience an effort tending to make it rotate, according to Weber; while according to Riemann this effort would be three times more powerful.

2. Rotary oscillations of an insulated magnet of large size, connected to earth by the end of its axis at the instant of its highest speed. According to Riemann alone, the magnet would be electrically charged when it came back to rest.

3. Rotation of a strongly electrified disc, similar to that used in Rowland's experiment, in front of a ring of conducting wires the meridian of which passes through the axis of rotation. According to Weber alone, a permanent current would circulate in the ring.

4. Rotation of a circular multiplying coil in a magnetic field. If the axis of rotation were horizontal, a light electrified body, suspended in the horizontal plane of the axis, would remain motionless, according to Riemann and Clausius, and would be deflected according to Weber.

### **E. COLARDEAU—MAGNETIC IMAGES PRODUCED BY FEBBLY MAGNETIC BODIES.**

(*Bulletin de la Société Internationale des Electriciens*, Vol. 4, p. 188, 1887; *Journal de Physique*, Vol. 6, p. 83, 1887.)

If iron filings are shaken on to a very thin piece of sheet iron placed on the poles of a magnet, the particles arrange themselves along the lines of force. But quite a different result is produced by shaking very carefully on to the sheet of iron sesquioxide of iron, iron scales from forgings—which are a mixture of ferrous and ferric oxides—and the various oxides of nickel and cobalt, all in a state of very fine powder. Instead of using dry powders, the oxides may be suspended in a varnish of lac dissolved in alcohol, which on drying preserves permanently the image formed. The powder accumulates in considerable heaps corresponding to the edges of the poles of the magnet, and above the intermediate space it arranges itself in lines or threads, the formation of which is facilitated by tapping the sheet of iron. These threads do not arrange themselves along the lines of force as do iron filings, but at right angles to them, or, in other words, they map out the equipotential lines. No results have been obtained with diamagnetic powders, using the strongest magnets at the author's disposal.

A double effect is produced with magnetic oxide of iron in fine powder, and with iron, nickel, and cobalt reduced by hydrogen to the metallic state. There is a double system of interlaced threads, the one following the lines of force, and the other the equipotential lines. This double system can be most readily obtained with the solution of lac.

The phenomenon is no longer observed if too thick a sheet of iron is used, nor if the iron sheet is replaced by one of a non-magnetic metal such as copper or zinc.

The author explains the phenomenon by the property, possessed by all lines of force, of tending to shorten themselves. The curved threads of powder formed along the lines of force tend to shorten themselves, and to approach the position of maximum intensity, where they would lie in a straight line from one pole to the other. But this tendency is opposed by the attraction of the neighbouring particles which are magnetised, and by friction on the surface of the iron sheet; in the case of feebly magnetic bodies this latter force is the more powerful, and the particles, in moving to the position in which the threads will be shortest, leave a sort of trail behind them arranged along the equipotential lines, and formed of small particles of the powder heaped up one behind the other.

### **BERTHOFF—TELEPHONE LINE BETWEEN PARIS AND BRUSSELS.**

(*Bulletin de la Société Internationale des Electriciens*, Vol. 4, p. 191, 1887.)

The new line, 199 miles long, consists of two silicon bronze wires 3 mm. in diameter (11 L.S.G.), and having a resistance of 3·86 ohms per mile, or 1,550 ohms for the double line. The conductors are carried on the same poles

as the ordinary telegraph wires, and are crossed at each pole. The cost of erection was about £4,000. The wire weighs 22½ pounds per mile, and has a breaking strain of 28½ tons per square inch.

The author prefers an independent line for telephony to the Van Bysselbergh system, as the latter interferes with the use of rapid telegraph instruments owing to the introduction of the compensating electro-magnets, and necessitates the use of anti-induction apparatus for all electric apparatus such as bells, electric semaphores, &c.

The problem of long distance telephony can be completely solved by the use of aluminum bronze wire of sufficient size. Thus a wire of 5 mm. diameter (about 5 L.S.G.) would enable conversation to be carried on between New York and Chicago—a distance of 994 miles. On submarine cables it is only possible to talk over about 20 nautical miles.

### **COLARDEAU—EFFECT OF MAGNETISM ON CHEMICAL REACTIONS.**

(*Journal de Physique*, Vol. 6, p. 129, 1887.)

A very similar phenomenon to that described in a preceding abstract is noticed, if a solution of copper sulphate be poured on to a sheet of iron placed on the poles of a horse-shoe magnet. In the case of a plate  $\frac{1}{10}$ th mm. thick, the deposit of copper on the space between the two poles and just round them presents the appearance of a net formed of links of dull copper, and separated by spaces of bright metal. These latter portions of the deposit are below the general level, as are those round the edges of the poles. These links, as in the case of the fine iron dust, follow the equipotential lines of the field, and not the lines of force. The same effect may be produced with a solution of any metallic salt which is reduced by iron.

The simplest explanation seems to be that electric currents are set up by the chemical action of the iron on the salt of copper, and that these currents, forming closed circuits in the liquid, are acted upon by the magnet. That this explanation is not sufficient can be shown at once by placing a very thin plate of zinc above the iron, and pouring on it a solution of acetate of copper, which is reduced by zinc. No arrangement of the copper deposit is to be seen.

The similarity in the appearance of the copper deposit with the arrangement of the feebly magnetic powder in the former experiments would seem to point to the probability of an intimate relation between the causes of the two phenomena. The arrangement of the copper deposit is probably due to the formation of molecules of sulphate of iron, which, being feebly magnetic, arrange themselves along equipotential lines, and thus protect these lines from a further deposit of copper. This view is supported by two facts. If the solution is disturbed or agitated in any way, the deposit is uniform. If an alcoholic solution of copper bichloride is used, which is a brilliant green, whilst the corresponding iron salt is yellow, the formation of the yellow lines of iron chloride may be watched as it gradually develops.

**Professor E. HAGENBACH-BISCHOFF**—DETERMINATION OF THE SPEED OF PROPAGATION OF ELECTRICITY IN TELEGRAPH WIRES.

(*Annalen der Physik und Chemie*, Vol. 29, p. 377, 1886; *Journal Télégraphique*, Vol. 9, p. 6, 1885, Vol. 10, p. 266, 1886, Vol. 11, pp. 9 and 29, 1887.)

The instrument used for the measurement of the time was the comparator of Lissajous, by means of which the difference of phase of two tuning-forks, vibrating in planes at right angles to each other, can be determined. The slightest alteration in the difference of phase is at once shown by an alteration in the shape of the figure. The first tuning-fork was placed with its axis in a horizontal plane, and the plane of its vibrations was vertical; to one limb of this fork was fixed a small piece of tinfoil, which could be brilliantly illuminated. The second fork, adjusted to exactly the same number of vibrations as the former, was fixed with its axis vertical, and so that the plane of its vibrations was horizontal. One limb carried the object-glass of a microscope, through which the brightly illuminated tinfoil could be observed. The two tuning-forks were joined up in circuit with a battery and the line to be experimented upon; and by means of a special mercury commutator the line could be put into circuit after the two forks, or between them. In the former case the figure of Lissajous kept its normal shape; but in the second case, when the current took time to pass through the line from one fork to the other, the shape of the figure was altered.

In order to judge of the amount of this alteration the section of an ellipse was drawn on the surface of a vertical glass cylinder, and by rotating this cylinder more or less on its vertical axis the figure given by the tuning-forks could be reproduced. The angle through which the cylinder was turned was thus a measure of the retardation of the current.

The experiments were made at Bâle over circuits looped at various stations. These circuits did not consist of the same sized wire throughout, but the various sizes were all reduced to equivalent lengths of 4 mm. iron wire. The results may be tabulated as under.—

Line looped at	Reduced Length Kilometres.	Rotation of Cylinder.	Retardation. Sec. mcs.	$10^4 \frac{t}{\mu}$
Lucerne	284.8	81°	0.00176	217
Olten ...	157.5	24°	0.00052	210
Sissach ...	115.8	14°	0.00030	226
Laestel ...	97.6	10°	0.00022	227

The author deals at length with the theory of the question, and he states as the "law of charge" that the duration of the charge is independent of the absolute value of the potential; and with respect to different wires submitted to the same relative limiting conditions, the duration of charge is proportional to the square of the length of the wire, and to the capacity and resistance



of unit length. This is shown in the last column. The article also contains a review of all previous experiments by other persons on the same subject.

### L. ARONS—METHOD OF MEASURING THE COUNTER E.M.F. IN THE ELECTRIC ARC.

(*Annalen der Physik und Chemie*, Vol. 30, p. 95, 1887.)

The method of measurement is a modification of that adopted by Cohn for the determination of the polarisation of a cell, and consists of a particular arrangement of a Wheatstone bridge. Two branches contain each a fixed resistance; the third a variable resistance in the fourth are joined up in series a battery of accumulators, a safety fuze, a key, a variable resistance, an ammeter, and the arc lamp, the carbons of which can be short-circuited, while the regulating solenoid still remains in the circuit. One diagonal contains a Siemens torsion galvanometer, the fixed coil of an electro-dynamometer, and a variable resistance. The other diagonal contains the secondary coil of an induction apparatus. The circuit of the swinging coil of the electro-dynamometer is completed through another secondary coil, the primary of which is in circuit with a Dubois apparatus, and with an ordinary interruptor.

A measurement is first taken with the arc in circuit, and the resistances are so arranged that the deflection of the electro-dynamometer remains constant when the induction apparatus is set to work. Then the carbons are short-circuited, and an equivalent resistance inserted in place of them. Only two measurements are given in the paper: in one, with a current of 3.4 ampères and an E.M.F. of 40.6 volts, the resistance of the arc was 2.1 ohms; in the other, with 4.1 ampères and 39.6 volts, the resistance was 1.6 ohm.

(NOTE. No mention is made of the length of the arc in either case.)

### W. von ULJANIN—AN EXPERIMENT OF EXNER'S ON THE CONTACT THEORY.

(*Annalen der Physik und Chemie*, Vol. 30, p. 699, 1887.)

Exner having made some experiments, the results of which confirmed him in his opinion that the contact theory is untenable, the author was led to repeat them. The experiment consists in determining the alteration in potential of an insulated metal plate when its capacity is altered. In the author's arrangement a round brass disc, carried on an insulating stand, was connected to earth, the disc was surrounded by a metal case in two halves, also connected to earth. By means of a commutator the disc could be disconnected from earth and put on to an electrometer. No deflection was remarked by Exner under any circumstances; but the author found that when the surrounding case was withdrawn from the disc, whilst the latter was in connection with the electrometer, the needle was deflected. Summar

experiments were also made with two concentric zinc cylinders, the outer one of which could be raised or lowered by means of a cord passing over a pulley. The author concludes that, far from the experiment being opposed to the contact theory, it is in direct support of it.

**F. KÄGI—RESEARCHES ON THE ELECTRICAL BEHAVIOUR OF MICA AS AN INSULATING MEDIUM IN CONDENSERS.**

(*Beiblätter*, Vol. 10, p. 625, 1886.)

From experiments with a mica condenser, in which the author determined the relation of the quantity of electricity discharged to the amount undischarged from the previous charge, the duration and strength of which could be varied, he concludes that mica is unsuitable for use in standard condensers. Some experiments on the resistance of mica would appear to lead to the conclusion that it conducts like an electrolyte.

**R. BLÄNSDORF—HERMETICALLY SEALED BATTERIES.**

(*Beiblätter*, Vol. 10, p. 630, 1886.)

The cell is made of ebonite, and is closed by an ebonite lid with a washer of soft india-rubber. The upper part of the cell is split up into several divisions in which are placed the carbons and zincs; the lower part contains the exciting fluid. The battery is put into action by reversing it. In the ebonite lid are apertures closed by thin sheets of india-rubber. These allow of the expansion of the gas produced in the battery, without any risk of damage to the ebonite cell, and without the acid being forced up through the vent, as is the case in other hermetically sealed batteries.

**A. BATTELLI—INFLUENCE OF MAGNETISATION ON THE THERMAL CONDUCTIVITY OF IRON.**

(*Beiblätter*, Vol. 10, p. 780, 1886; *Atti dell' Accademia di Torino*, 21, p. 559, 1886.)

Two tubes, one of soft Swedish iron, and one of brass, were fixed in the sides of tin cubes, through which steam could be led. Holes 5 cm. apart were bored in the bars, into which the junctions of a thermo-element could be inserted. A very powerful electro-magnet was brought within 7 mm. of the free end of the iron bar. So long as the bar was cold, no deflection on the galvanometer was observed, on heating the bar, alternate very slight positive and negative results were produced.

A compound bar was then tried, made up of a square copper bar, a short square iron bar, and a second copper bar. One copper bar was fixed in the heated cube, and the ends of the thermo-element were placed in holes in the copper bars. The iron bar was surrounded by a coil. At first this coil was wound with 1·7 mm. wire; but as it was somewhat heated by the passage of the current and radiated heat on to the thermo-element, this winding of wires was

replaced by one of copper strip. On heating one end of the compound bar, and at the same time passing a current round the iron portion so as to magnetise it, the hot junction was cooled, while the cool junction was heated, showing that the magnetisation had caused a decrease in the conductivity of the copper.

## **H. WILD—DETERMINATION OF THE COEFFICIENT OF INDUCTION OF STEEL MAGNETS.**

(*Beiblätter*, Vol. 11, p. 175, 1887.)

On the east side of the needle of a unifilar magnetometer is a magnetising coil, the axis of which is perpendicular to the meridian and cuts the centre of the needle; and on the west side a similar coil, but with less turns. On a current being led through the two coils, the action of the smaller almost compensates for that of the larger. If  $H$  is the horizontal force of the earth,  $e$  the constant of torsion of the suspension,  $I$  the current,  $c$  and  $f$  the constants of the coils with respect to the needle, and  $\phi$  the small uncompensated deflection, then

$$H(1 + e) \tan \phi = I(c - f).$$

After breaking the circuit the magnet to be experimented upon is laid in the magnetising coils, and on the west side a magnet as similar as possible is brought up to the needle until only a very small deflection  $\phi'$  is produced. The current is then passed through both coils so as to increase the action of the magnet, and the deflection  $\phi''$  is noted; finally, the current is reversed, and the deflection  $\phi'''$  is noted. The current is at the same time measured on a tangent galvanometer; and then, knowing the temperature coefficient of the magnet, the changes in the earth's magnetism, the moments of the magnets, &c., we can deduce the change produced in the moment of the magnet lying in the spiral by a specified external force. For coincident and opposed action of the external forces acting on the permanent magnet, Lamont's rule brings out the difference of the two actions much too high, it is, moreover, not nil for weaker forces acting for a longer time, as would follow from the experiments of F. Kohlrausch and Sack with induction currents.

## **J. BORGMANN—SOME EXPERIMENTS ON THE PROPAGATION OF ELECTRICITY THROUGH AIR.**

(*Beiblätter*, Vol. 11, p. 182, 1887; *La Lumière Electrique*, 22, pp. 193 and 246, 1886.)

One electrode of a Holtz machine is connected to earth, the other is joined by a short wire to an insulated Bunsen burner. Opposite to the Bunsen burner is a spirit lamp which has a platinum wire twisted round its wick, the other end being connected through a galvanometer to earth. On working the Holtz machine, the galvanometer shows that a current passes through the air from the Bunsen burner to the spirit lamp. On increasing the distance between the two from 0.305 m. to 2.54 m. the deflection of the galvanometer fell from 300 divisions to 2 divisions.

A second spirit lamp and a second galvanometer may be introduced into the circuit before the earth, and approximately the same deflection will be observed on both galvanometers. Though there are two air gaps, the circuit behaves as though it were entirely closed. Much feebler, but similar, results were obtained with 120 copper-zinc cells in place of the Holtz machine. The experiment may also be made with an induction coil and telephone.

It is also possible to obtain a direct deflection of the needle of a galvanometer, without making any use of its own coil, if two insulated lamps are placed one north and one south of the galvanometer, and connected by platinum wires to earth and to the Holtz machine respectively.

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**W. KOHLRAUSCH—USE OF THE SIEMENS TORSION GALVANOMETER FOR THE DIRECT MEASUREMENT OF STRONG CURRENTS.**

(*Centralblatt für Elektrotechnik*, Vol. 8, p. 313, 1886; *Le Luminère Electrique*, Vol. 23, p. 276, 1887.)

In order to obviate the use of a shunt, which introduces errors owing to the alteration of its resistance due to the heating effect of the current, the author uses a torsion galvanometer set up on a board, on which is fixed an independent coil of thick wire on each side of the galvanometer. By this means it is possible to measure currents up to 70 ampères, when the measurement of the current is being made, the extra coils alone are used, the fine wire coils of the galvanometer itself being out of circuit. The additional coils must be placed with care so that the poles of the thumb magnet are exactly on the axis of the coils, and exactly in the centre between them. To effect this, one of the coils is provided with a screw adjustment. The modified instrument is calibrated by comparison with a standard, the coils being first connected in series so as to have a double action on the magnet, and then the connections of one reversed so as to have no action. By a simple arrangement of mercury cups and copper bridge-pieces it is easy to change the connections from the coils of the galvanometer proper to the additional coils, so that either the current or the difference of potential may be measured on the same instrument.

# LIST OF ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM,

Appearing in some of the principal English and Foreign Technical Journals  
for the months of APRIL and MAY.

(*Philosophical Magazine*, Vol. 23, No. 143, April, 1887.)

**Prof. S. P. THOMPSON**—Arc Lamp for Use with the Duboscq Lantern.

**H. H. M. BOSANQUET**—The Law of the Electro-Magnet and the Law of the Dynamo.

(No. 144, May, 1887.)

**H. H. M. BOSANQUET**—Determination of Coefficients of Mutual Induction by means of the Ballistic Galvanometer and Earth-Inductor.

**W. BROWN**—Effects of Percussion and Annealing on the Magnetic Moments of Steel Magnets.

PROCEEDINGS OF THE ROYAL SOCIETY

(*Nature*, April 7, 1887, p. 549.)

**C. V. BOYS**—The Radio-Micrometer.

(*Nature*, April 14, 1887, p. 574.)

**H. LAMB**—On Ellipsoidal Current Sheets.

(*Nature*, April 21, 1887, p. 598.)

**Dr. ALDER WRIGHT**—Development of Voltaic Electricity by Atmospheric Oxidation.

(*Nature*, April 28, 1887, p. 632.)

**Prof. J. A. EWING**—Magnetisation of Iron in Strong Fields.

(*Comptes Rendus*, Vol. 104, No. 14, April 4, 1887.)

**LIPPMANN**—Stroboscopic Method of Comparing the Time of Vibration of Two Tuning-Forks. **G. MANEUVRIER** Starting the Electric Arc without Contact of the Electrodes. **H. MERCADIER**—Theory of the Telephone, and its Transformation into an Electro-magnetic Resonator.

(No. 15, April 12, 1887.)

**E. BRANLY**—New Method of Using the Thermo-electric Multiplier.

(No. 16, April 18, 1887.)

**LIPPMANN**—A New Absolute Unit of Time. **H. PELLAT**—Measurement of the Potential Difference of Two Metals in Contact.

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**COLLADON**—An Exceptional Lightning Stroke. **C. DECHARME**—Isogonic Magnetic Curves.

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**OMIUS**—Electric Disturbances caused by the Earthquake of February 28.**P. H. LEDEBOER**—Flow of Magnetic Induction in the Magnets of a Dynamo. **C. LAGRANGE**—Causes of the Diurnal Variations of Terrestrial Magnetism.

(No. 20, May 16, 1887.)

**MASCART**—Action of Earthquakes on Magnetic Instruments. **C.****LAGRANGE**—Diurnal Variations of the Terrestrial Magnetism in the Tropics, and the Secular Variations.

(Annales Télégraphiques, January—February, 1887.)

**WÜNSCHENDORFF**—Operations for the Repair of the Marseilles-Algiers Cable, 1880-81. **M. LEVY**—The Electrical Transmission of Power between Creil and Paris. **A. REYNIER**—The Use of Cofferdam in Batteries.

(Journal de Physique, Vol. 8, April, 1887.)

**H. PELLAT**—Absolute Electro-Dynamometer. **A. LEDUC**—New Method for Measuring Magnetic Fields.

(Vol. 6, May, 1887.)

**A. LEDUC**—Study of the Magnetic Field produced by an Electro-Magnet of Faraday.

(La Lumière Electrique, Vol. 24, No. 14, April 2, 1887.)

**ABDANK-ABAKANOWICZ**—Study of Integragraphs. **P. H. LEDEBOER**—Predetermination of the Characteristic of a Dynamo. **J. SARCIA** and **E. SARTIAUX**—Experimental Investigation of Magnetic Fields. **A. PALAZ**—Continuous Currents in Telephonic Cells. **GOUY**—Standard Cell. **Dr. R. ULBRICHT**—The Work of the Current on Telegraphic Lines. **J. WETZLER**—Automatic Cut-out for Accumulators.

(No. 15, April 9, 1887.)

**L. PALMIERI**—Production of Electricity by the Condensation of Aqueous Vapour. **ABDANK-ABAKANOWICZ**—Study of Integragraphs. **VASCHY**—Magnetic Sheets and Currents. **B. MARINOVITCH**—A New Electric Registering Apparatus for Meteorological Instruments. **J. SARCIA** and **E. SARTIAUX**—Experimental Investigation of Magnetic Fields. **E. DIEUDONNE**—Use of the Magneto-Inductor for Electric Bells. **A. PALAZ**—The System of Signals for Assistance on the Berlin Metropolitan Railway. **H. LE CHATELIER**—Measurement of High Temperatures by Thermo-electric Couples. **E. M.**—Weinhold's Portable Galvanometer. **ELSASS**—Nobili's Rings, and the Electro-chemical Phenomena which produce them. **A. EBELING**—On Thermo-electric Force between some Metals and the Solutions of their Salts. **J. WETZLER**—Recent Improvements in Apparatus for Regulating Temperature. **J. WETZLER**—New Combined Sounder and Telephone.



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- G. RICHARD**—Arc Lamps. **E. DIEUDONNÉ**—Use of the Magneto-Inductor for Electric Bells. **P. H. LEDEBOER**—New Military Sounder. **A. D'ARSONVAL**—Death from Electric Shocks. **G. MANEUVRIER**—Starting the Electric Arc without Contact of the Electrodes. **E. MERCADIER**—Electro-magnetic Resonator.

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(No. 18, April 30, 1887.)

- C. REIGNIER**—Self-regulating Dynamos. **A. D'ARSONVAL**—Instruments for the Study of Animal Electricity. **G. RICHARD**—Arc Lamps. **A. MINET**—Researches in Electrolysis. **E. DIEUDONNÉ**—New Arrangement of a Deprez-D'Arsonval Galvanometer. **LIPPMANN**—An Absolute Unit of Time. **H. PELLAT**—Measurement of the True Potential Difference of Two Metals in Contact. **O. BOCK**—Conductivity of Mixtures of KOH and KSH.

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- G. RICHARD**—Aluminium and its Electro-Metallurgy. **C. REIGNIER**—Self-regulating Dynamos. **TOMMASI**—Thermic Equilibrium in Electrolysis. **J. WETZLER**—Application of Electricity to Pneumatic Tubes.

(No. 20, May 14, 1887.)

- P. H. LEDEBOER**—The Flow of Magnetic Induction in the Field Magnet of a Dynamo. **A. DECHARME**—Isogonic Magnetic Curves. **J. SARCIA and E. SARTIAUX**—Experimental Investigation of Magnetic Fields. **H. WEBER**—Theory of the Wheatstone Bridge. **S. KALISCHER**—Production of an E.M.F. in Selenium by the Action of Light. **J. ELSTER and H. GEITEL**—The Electrification of Gases by Incandescent Bodies. **E. VILLARI**—The Emissive Power of Electric Sparks, and their Appearance in Different Gases. **G. G. GEROSA**—Resistance of Mixtures of Amalgams. **J. WETZLER**—New System of Writing Telegraph.

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- A. PALAZ**—Effect of Electro-Magnets on Telephone Lines. **MASCART**—Effect of Earthquakes on Magnetic Apparatus. **GESTERREICH**—Automatic Commutator for Telephone Lines. **MARANGONI**—New Relations between Light and Electricity.

(Bulletin de la Société Internationale des Electriciens, Vol. 4, No. 37, April, 1887.)

- C. CHARDIN**—Automatic Cut-out on a New Principle. **NAPOLI**—Pollak and Binswanger's Electric Tramway. **PARENTHOU**—Apparatus for Indicating and Registering at a Distance. *Anon.*—Speed-Regulator. **G. MANEUVRIER**—New Means of Starting the Electric Arc without putting the Electrodes in Contact. **P. H. LEDEBOER** and **G. MANEUVRIER**—Determination of the Coefficient of Self-Induction. **MASCART**—Determination of the Poles of Magnets. **GOUY**—Standard Cell.

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- MERCADIER**—Theory of the Telephone, and its Transformation into an Electro-magnetic Resonator. **DE LA TOUANE**—Telephone Line between Paris and Brussels. **D. COLLADON**—Lightning Stroke at Schoren. **V. MÉSÉROLE**—Accumulators with High E.M.F. **E. GÉRARD**—Magnetic Dynamometer. **H. DUFOUR**—Action of Magnetism on the Velocity of Efflux of Liquids. *Anon.*—Automatic Electric Governor of the Speed of Trains. **POUSSEREAU**—Decomposition of Perchloride of Iron by Water. **LIPPMAHN**—Stroboscopic Method of Comparing the Duration of Two Vibrations. **LIPPMAHN**—A New Absolute Unit of Time.

(Journal Telegraphique, Vol. 11, No. 4, April, 1887.)

- ROTHEN**—Telephony (*continued*). **GATTINO**—Duplex and Quadruplex Systems of Transmission (*continued*).

(No. 5, May, 1887.)

- ROTHEN**—Telephony (*continued*). **VIANISI**—Duplex Methods of Telegraphy.

(Annalen der Physik und Chemie, Vol. 31, Pt. 1, No. 5, 1887.)

- S. KALISCHER**—Production of E.M.F. by Light. **J. ELSTER** and **H. GRITEL**—Electrification of Gases by Incandescent Bodies. **E. PFEIFFER**—A Modification of Kohlrausch's Sine Inductor. **H. LORBERG**—Electro-Dynamics. **O. MUND**—Determination of the Poles of Induction Machine.

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- C. L. WEBER**—Conductivity of Amalgams. **A. KOEPEL**—Determination of Magnetic Moments and Currents in Absolute Measure by means of the Balance. **W. KÖNIG**—Magnetism of Crystals. **H. CLAUDIUS**—Reply to Lorberg respecting Dynamos. **A. FÖEPL**—Electricity as an Elastic Fluid. **K. WESENDONCK**—Absence of a Polar Difference in Spark Discharges.

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- A. PALAZ**—Experiments on the Specific Inductive Capacity of some Dielectrics. **J. CURIE**—Specific Inductive Capacity and Conductivity of some Dielectrics. **G. G. GEROSA**—Resistance of Mixtures of Amalgams. **B. NEHEL**—Observations made on a Lalande Cell. **M. CORSEPIUS**—Passivity and Polarisation of Iron. **P. DUHEM**—Electric Pressure and Electro-capillary Phenomena. **H. PELLAT**—Absolute Electro-Dynamometer. **A. WASSMUTH** and **G. A. SCHILLING**—Experimental Determination of the Work of Magnetisation. **M. STERNBERG**—Geometric Investigation of the Rotation of the Plane of Polarisation in the Magnetic Field. **A. RIGHI**—Experiments on the Reflection of Polarised Light from the Poles of a Magnet. **V. WIETLISBACH**—Self-Induction of Straight Wires. **J. BORGMANN**—Experiments on Induced Currents. **E. VILLARI**—Electric Discharges in various Gases. **J. BERTRAND**—Electric Units. **L. LUVINI**—Conductivity of Gases and Vapours. **F. EXNER**—Causes and Laws of Atmospheric Electricity.

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- F. AUERBACH**—Grouping of Batteries. **A. WEINHOLD**—Grouping of Batteries. **H. SLUGINOW**—System of Linear Conductors. **M. ZINGLER**—Insulating Material for Electric Leads. **H. LE CHATELIER**—Measurement of High Temperatures by Thermoelectric Couples. **A. v. ETTINGSHAUSEN** and **W. NEERNST**—Hall's Phenomenon. **A. v. ETTINGSHAUSEN**—Measurement of the Hall Effect on the Differential Galvanometer. **C. MARANGONI**—Paramagnetism and Diamagnetism. **F. KOHLRAUSCH**—Magnetism of the Human Body. **F. EXNER** and **P. CZERMAK**—Unipolar Induction. **J. BORGMANN**—Propagation of the Electric Current through Air. **C. MARANGONI**—Relation between Electricity and Light. **C. B. CROSS** and **W. E. SHEPARD**—Counter E.M.F. in the Electric Arc.

*Elektrotechnische Zeitschrift, Vol. 8, Pt. 1, April, 1887.*

- F. von HEFNER-ALTENECK**—A New Dynamo of Siemens & Halske. **Dr. E. STRECKER**—Kohlrausch's Spring Balance for Weak Currents. **Dr. O. FROLICH**—Theory of the Dynamo. **C. L. R. E. MENGES**—Automatic Regulation of Electric Work. **H. SESEMANN**—Calorimetric Current and Potential Meter. **Dr. BOERNS**—Experiments of the Franklin Institute. **Dr. A. TOBLER**—Siemens & Halske's Atlantic Cable. **C. E. McCulloch's** Fire-Alarm Telegraph System. **Dr. F. NRESEN**—Electro-magnetically driven Tuning-Forks. **W. KOHLRAUSCH**—Electric Governor for Gas Engines. **W. OESTERREICH**—Automatic Conclusion Signal for Telephone Lines. **K. WIESNER**—The Moderating Effect of Electro-Magnets in Telephone Circuits.

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- Dr. O. FROLICH**—Optical Demonstration of the Action of a Telephone.  
**Dr. O. FROLICH**—Theory of the Dynamo. **J. L. HUBER**—Electric Tramways in Hamburg. **W. KOHLRAUSCH**—Notes on Accumulators.  
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# NOTICE.

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# JOURNAL

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The One Hundred and Sixty-ninth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 10th, 1887—Sir CHARLES T. BRIGHT, M. Inst. C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and ordered to be suspended.

The SECRETARY announced that the Council had approved the following transfers :—

From the class of Associates to that of Members—

J. J. Allen.		Alfred E. Mills.
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From the class of Students to that of Associates—

C. Ashmore Baker.		Bertram Thomas.
Charles Garrard.		C. H. Wordingham.

Donations to the Library of the Society were announced as having been received since the last meeting from the Astronomer-Royal; the Institution of Civil Engineers; W. Ellis, Member; Professor J. Ewing; Brent Good, Esq.; Messrs. Chas. Griffin & Co.; Dr. Edward Hopkinson, Member; the India-Rubber, Gutta-Percha, and Telegraph Works Co., Limited: H. R. Kempe,

Member; J. D. Mullins, Esq.; the New York Agent, College of Electrical Engineering; Anthony Reckenzaun, Esq.; Sir David Salomons, Member; The Commander F. Salvatori, Foreign Member; T. F. Tofts, Associate; F. Uppenborn, Esq.; Messrs. Whittaker & Co.; and J. H. Wunschendorff, Foreign Member; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: Our paper this evening is one which scarcely comes within our usual debating ground, namely, Electricity; but its subject forms a very important part in the preparations for laying submarine cables. Submarine cables have almost entirely been constructed and laid by English enterprise, and the work has been carried out to such an extent that there is scarcely a port of any importance in the world which does not possess communication by submarine telegraphs. In the early days of submarine cables they were principally laid in comparatively shallow water; the deepest water for some years crossed, being that in laying a cable from Portpatrick, in Scotland, to Donaghadee, in Ireland, in 1853. In that case we had to pass over 150 fathoms, dropping down from 34 fathoms in the immediate neighbourhood. No special soundings had to be made in those days for cable laying, as the work had all been done before by the naval surveyors, and the soundings were all marked down upon the charts, together with the nature of the bottom—whether ooze, sand, or rock. When, however, a few of us set to work to lay a cable across the Atlantic, we had to grapple with another kind of business. A small number of soundings—some of them altogether unreliable—had then (in 1855) been made in the vast depths of the ocean, from which is reflected in calm weather a tint “so blue, so pure, and so beautiful,” that no other can bear comparison with it; it is a colour only obtained from water so deep as to have been termed by mariners in those times “unfathomable.” Prior to commencing the first Atlantic line two lines of soundings were taken over the proposed route by H.M.S. “Cyclops” and the U.S.S. “Arctic.” Since then there have been many lines of soundings taken for cable purposes in “blue water,” and we are going to hear some more of them from Mr. Stalhbrass, who is about to read a paper in which

he will show the necessity of having such soundings made more closely than in old times, because frequently, as even on the "telegraphic plateau" which Capt. Maury spoke of,\* banks of a dangerous character for the durability of cables have been found by their means. Mr. Stallibrass will give us examples of these banks in the diagrams accompanying his paper. Prior to that time I may say that our deep-sea soundings were in many cases altogether unreliable. For example, Capt. Denham, in H.M.S. "Herald," reported a sounding with bottom in the South Atlantic of 46,000 feet; but there is no doubt that his line was carried away by currents, or the ship drifted. Another sounding was given by Lieut. Parker, of the U.S. frigate "Congress," of 50,000 feet run out without bottom; but Admiral Richards, when Hydrographer to the Admiralty, eliminated from the Atlantic charts almost every sounding above 3,000, or at all events 4,000, fathoms, believing that there was no real bottom obtained above those depths in a vertical line. However, these are the things which will probably be remarked upon in the discussion which will doubtless arise upon the paper which I now ask Mr. Stallibrass to read.

The following paper was then read:—

### DEEP-SEA SOUNDING IN CONNECTION WITH SUBMARINE TELEGRAPHY.

By EDWARD STALLIBRASS, F.R.G.S., Member.

The connection between deep-sea sounding and submarine telegraphy is a very intimate one. Before the time of the first Atlantic Cable scheme in 1854, there seemed to be no practical value attached to a knowledge of the depths of the sea, and beyond a few doubtful results, obtained for purely scientific purposes, very little was known on the subject. With the Atlantic Cable scheme there arose at once the necessity for a more extensive knowledge of the bed of the Atlantic, and the history of deep-sea sounding may almost be said to date from that time.

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\* Maury's "Physical Geography of the Sea," p. 253, ed 1856.



In the report of the committee appointed in 1859 to inquire into the construction of submarine telegraph cables, the importance of soundings at frequent intervals over a cable route was well recognised; and, moreover, this report clearly pointed out that the failure at that early date of many of the cables already laid was due to want of knowledge of the bed on which they rested. An analysis of the composition of the sea bottom was also recommended.

At the time the first Red Sea cable was manufactured no soundings on its intended route existed, and it was only on the expedition for laying the cable reaching Karachi that the results of the work done by H.M.S. "Cyclops" were known. These soundings showed a maximum depth of 2,000 fathoms, while the cable and paying-out machinery had been designed for 1,000 fathoms.

The first Channel Islands cable failed chiefly because proper care was not taken in the selection of a route.

One of the objects of this paper is to bring before your notice how little has been done even up to now towards surveying the routes taken by our submarine telegraph cables. To this want of knowledge of the sea bottom, their short lives may in many cases be attributed.

Up to the present time the bed of the Atlantic—on which are laid, perhaps, the most important cables in the world—is very imperfectly surveyed. In the North Atlantic alone six important banks have already been discovered, and in the carrying out of the frequent repairs which become necessary to these cables unsuspected irregularities in the ocean bed have from time to time been discovered. Proper attention has not been given to some of the very steep inclines that occur in mid-Atlantic, and one might almost venture the opinion that there are many spots where, when carefully sounded, formations similar to the "Faraday Hills" would be discovered, and that some of the inclines in these cases are greater than that of the Irish coast, where special attention has been given to the gradients, and to which reference is frequently made.

Sea bottoms must not by any means be supposed to consist

entirely of mud. The earth's crust is made up of a good deal of rock, and soundings are apt to be misleading in this respect. I daresay many now present will remember the large volcanic boulders brought up by the s.s. "Faraday" from the bed of the Atlantic, which were exhibited in the Paris Electrical Exhibition of 1881.

It is a very difficult matter to judge of a bottom from soundings as usually plotted on a chart.

Let us suppose that a change in depth from 1,500 to 2,000 fathoms is found between two positions. In considering this I think one is apt to reason that a difference of 500 fathoms in 2,000 is equal to an increase of 25 per cent., whereas the depth at which this difference is found has really nothing to do with the question. There exists in reality a difference of level of 500 fathoms, or let us say 3,000 feet, which should be considered totally irrespective of the depth at which it occurs. When this difference occurs in deep water, it appears to the general mind very much less serious than when it occurs in shallow water, for the reason above stated. This difference may be caused by a gradual slope or, on the other hand, by an almost precipitous descent—a feature which is far from unusual.

For a just appreciation of variations in depth it is advisable to consider these variations in feet, as by so doing a ready comparison can be made with familiar mountain peaks, and the country surrounding them.

As soundings are taken, a profile of the bottom should always be plotted out. The vertical and horizontal scales in these profiles should be the same. Profiles in which the horizontal scale is many times larger than the vertical are very misleading to the ordinary observer, and even the educated eye often fails to truly appreciate diagrams of that description.

There is now no great difficulty in manufacturing perfect cables. As an instance, I may mention that last year 2,300 miles of cable were manufactured by one company, shipped out to the West Coast of Africa, and laid there, with as many as sixteen shore ends, without the least hitch either electrically or mechanically.

Quality is no longer a matter of difficulty, and in the future far more attention will probably be directed to the way in which cables are laid than to the details of their manufacture. Money expended in gaining a thorough knowledge of the bed on which cables are destined to lie will be more than repaid, if not during the actual laying of the cables, certainly in the prolonging of their lives.

With your permission I now propose to touch as briefly as possible on some portion of the history of deep-sea sounding, naming a few of the chief surveys which have been carried out; and I will then explain the method in which soundings were formerly taken, and that now adopted on a thoroughly equipped expedition.

It was not until the seventeenth century that anything of importance was done in deep-sea sounding. An ingenious apparatus was invented about this time by Hooke, and as his idea gave rise to our recent forms of sounding tubes I shall briefly describe the invention.

It consisted of a piece of light wood well varnished over, to which was attached a leaden weight sufficient to sink the wood. The apparatus was let go in the water, and on the lead striking bottom it became detached from the wood, which rose again to the surface. The depth was calculated from the time the wood was under water. This apparatus was no doubt fairly accurate in shallow water, but useless in great depths, where the enormous pressure waterlogged the wood and, by materially increasing its density, greatly diminished its speed of rising from the bottom. When used in currents the float was carried away and the record lost.

Several soundings were taken in deep water during the eighteenth century, but they were not of much value. The first, at all reliable, were made by Sir James Ross during his well-known Arctic expedition in 1818. Samples of the bottom were then brought up from depths of over 1,000 fathoms.

A great departure from the ordinary method of sounding was made in 1838 by the substitution of wire for hemp, which till this date was the material in general use for sounding.

This was during the first purely scientific survey by the Government of the United States, under Wilkes. The experiment proved a failure, and the use of wire was consequently abandoned. The reason for non-success, as afterwards found out, was owing to the employment of a wire so heavy that no indication of touching bottom could be observed without the use of such weights as were out of all reasonable proportion.

The year 1845 saw the commencement of the United States coast surveys. These have recently been extended to deep water, and have added immensely to our knowledge of subjects connected with submarine research.

The idea of using wire cropped up again in 1848, when Captain Barnett, R.N., on the suggestion of Lieut. Mooney, R.N., tried a sounding with iron wire varying in size from No. 1 to No. 5, the lightest wire being paid out first. This was not successful. The wire broke when 2,000 fathoms had been paid out. In the following year Lieut. Walsh, U.S.N., paid out 5,700 fathoms of wire in the Atlantic without apparently getting bottom.

It was the American Government which sent out the next important surveying ship, the "Dolphin." Her principal work was to disprove the existence of many of the reported shoals and rocks in the North Atlantic, which so hampered navigators. Fourteen soundings were taken. The results obtained led Captain Maury, U.S.N., when consulted by Mr. Cyrus Field on the first Atlantic Cable scheme, to state: "The bed of the sea between Ireland and Newfoundland is a plateau which seems to have been placed there especially for the purpose of holding the wires of a submarine telegraph, and of keeping them out of the way." I am afraid this is a prophecy which later experience has proved to be, to a very great extent, incorrect. Maury, however, should hardly be blamed for this opinion, as the information upon which he based his statement was very incomplete. A less sweeping assertion would, however, have been more prudent. I take this opportunity to point out on what small data this report was based, because I think that on several occasions this report has not been fully

understood, and has damaged projects of transatlantic telegraphy by routes other than between Ireland and Newfoundland.

The importance, however, of more soundings was very evident, and the "Arctic" was lent by the United States Government for this purpose, under Lieut. Berryman, U.S.N. Massey's sounding machine was used, and 24 soundings were taken. These seemed to bear out Maury's idea of a "plateau," and "telegraph plateau" came to be a generally accepted term.

The same ground was gone over by H.M.S. "Cyclops" in 1857, and 34 more soundings were taken.

In the following year the proposed Azores route was sounded by H.M.S. "Gorgon," with a result which did not seem satisfactory. This route was therefore abandoned, and in 1860 a survey of the Northern route, between the Faroe Islands, Iceland, Greenland, and Labrador, was made by Admiral Sir Leopold McClintock in H.M.S. "Bulldog."

In 1862 H.M.S. "Porcupine" was despatched to investigate the supposed sudden dip from 550 to 1,750 fathoms about 170 miles west of Valentia, when the "Porcupine Bank" was discovered.

During the efforts to recover the first Atlantic cable in 1860, after its failure, evidence of a far worse bottom than had been expected was clearly shown; and additional soundings being strongly recommended, the British Government sent out surveying ships, and by the time the second cable was laid there were some 57 soundings over a line 1,700 miles in length, or about one every 30 miles.

The stimulus given to deep-sea research by submarine telegraphy lasted for a considerable time. In 1868-69 H.M.S.S. "Lightning," "Porcupine," "Hydra," "Gannet," "Valorous," and others, were at work, and the Swedish ships "Sophia" and "Josephine," the latter discovering the "Josephine Bank" some 250 miles west of Cape St. Vincent.

The U.S.S. "Yantic" in 1870 took 134 soundings in the West Indies, and in 1871 the "Mercury" ran a line of 15 soundings between Sierra Leone and Havana.

In 1872 the use of wire in place of hemp for sounding lines

was again revived. Sir William Thomson was convinced of the suitability of steel wire for the purpose, and, being recommended pianoforte steel wire, tried a sounding with this from his yacht, the "Lalla Rookh," in the Bay of Biscay. This sounding was perfectly successful, and in a paper read before this Society in April, 1874, Sir William Thomson described the method employed.\*

The Silvertown Company at the end of 1872 took some of the soundings required for the laying of the first Lizard-Bilbao cable with wire. This was the first practical use made of the new method.

The United States Navy now came to the fore again, and after improving very much on Sir William Thomson's machine, employed his system of sounding on the "Tuscarora" during her work in 1873-74. The success achieved by Commander Belknap on this expedition has never been surpassed. No less than 120 consecutive casts were made in the deepest water of the Pacific without a single accident.

The "Challenger" Expedition, which is the most important submarine exploring expedition ever sent out, started in 1873, and in that and the three following years 504 soundings were taken. An immense amount of most valuable scientific work was done, which, however, does not come within the scope of my subject.

While this expedition was away, the United States Government was not idle. Its survey ship "Blake" was at work under Captain Sigsbee, and later under Captain Bartlett, and between the years 1874 and 1879 3,195 soundings were taken by her, mostly in deep water. This immense number is more than has ever been taken by any other single ship. Steel wire was used both for sounding and for dredging, and with it more rapid work was done in a small ship of only 350 tons burden, with a complement of 45 hands all told, than in the "Challenger" of 2,000 tons, and a proportionately larger crew. The machine used was

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\* "On Deep-Sea Sounding by Pianoforte Wire," by Sir William Thomson. See Journal of the Society, part 8, 1874.



designed by Captain Sigsbee, who at the close of the work wrote a most valuable volume on the subject.

In 1876 the "Gettysburg Bank" was discovered by a United States man-of-war of the same name. The French ships "Travailleur" and "Talisman" have done important work since that date, although principally engaged in making collections of various objects—animal, vegetable, and mineral—with which the sea abounds. The valuable work done by the Italian Navy also, in recent years, should not be forgotten.

The enterprise of the British Government in the direction of marine research seems to have exhausted itself in the "Challenger" Expedition, for since then little has been done by it. Private companies, however, have to some extent kept up the national reputation, and since 1876 a great deal of valuable work has been done.

I have forgotten to mention that in 1875, during the preliminary survey for the West Coast of America Telegraph Company's cables, 458 soundings were taken. Particulars of this work will be found in a communication made to the Society by Mr. H. Benest in 1877.\*

Messrs. Siemens Bros. took many soundings in the North Atlantic in 1874, 1879, 1881, and 1882, and discovered the so-called "Faraday Hills;" indeed, ten times more sounding was done for the Atlantic cables laid by them than for all the others put together. I very much regret that I am unable to enlarge on this very valuable work; but my time is limited, and I have yet to mention certain sounding expeditions at which I personally assisted, and which I am consequently better able to describe.

A considerable number of soundings have also been taken in various parts of the world by the Telegraph Construction and Maintenance Company, and also by the Eastern Telegraph Company. Most of these soundings are to be found on the charts issued by the Admiralty, and call for no special remarks.

It was in the year 1881 that the importance of a thorough

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\* See Journal of the Society, part 19, 1877.



*preliminary* survey was first fully recognised by a telegraph company. The India-Rubber, Gutta-Percha, and Telegraph Works Company, better known as the Silvertown Company, obtained a contract from the Central and South American Telegraph Company, of New York, for the establishment of cables along the west coast of America, from Lima, in Peru, to Tehuantepec, in Mexico—a total length of somewhere about 2,500 miles. The existing surveys of this coast were very poor, and there were absolutely no soundings beyond a very short distance from shore. The repairing steamer "Retriever" was chartered from the West Coast of America Telegraph Company, and sent out to survey the route for the intended lines of cable. It was on this expedition also that the importance of a knowledge of the composition of the sea bottom was first fully recognised. An analytical chemist accompanied the vessel during the whole survey, and all samples of deposits were tested by him for substances which might prove injurious to the sheathing of cables, and to avoid which it might be necessary to change the route or alter the type of cable.

The great value of a careful survey was clearly shown during this expedition, and 739 soundings were taken before the routes for the cables and the distribution of the several types of cable were finally decided on.

The next important survey I have to mention is one made in 1883 by the same Company before laying the cables which connect the Canary Islands with Spain. An interesting account of this expedition was given in the *Times* of the 7th December of the same year.

This part of the Atlantic, supposed to be the site of the lost Atlantis, is not unreasonably regarded as a suspicious one.

For the first Lisbon-Madeira cable no complete survey was made, and only a rough estimate of the depth was obtained by a line of soundings 25 miles apart. The result of this was that the bottom seemed to present a fairly level surface, with an average depth of water of about 2,000 fathoms. No attention appears to have been paid to apparent irregularities of 300 or 400 fathoms, or, let us say, 1,800 or 2,400 feet. In laying the cable, however, a bank was found with only 100 fathoms of water on it where

2,000 fathoms had been expected, and through a sufficiency of slack not having been paid out the cable was suspended in festoon and broke astern of the ship. This shoal is marked on the Admiralty charts as the "Seine Bank," and was so called from the ship that discovered it.

From a utilitarian point of view this accident is an instructive one, as the amount of money spent in the recovery of the lost cable was incomparably greater than the expense of a preliminary survey could have been. Soon after this the Josephine and Gettysburg Banks were discovered.

Profiting by previous experience, the Silvertown Company determined, if possible, to secure a good bottom for the cables they had contracted to lay for the Spanish National Submarine Telegraph Company, and an exceedingly careful and extensive survey was made of the ocean bed between Spain and the Canaries. The ships employed on the work were the telegraph steamers "Dacia" and "International." These vessels carried sufficient cable to connect the islands of La Palma and Gran Canaria with Tenerife, and Tenerife with Cadiz.

I propose to give some details of this expedition, which I accompanied, as it may be considered a typical one.

The ships met at Cadiz, and, after landing the shore end there, started sounding on the 4th October, 1883, having been joined on the previous day by Mr. J. Y. Buchanan, F.R.S., who is well known in connection with the "Challenger" Expedition.

The general programme was for the "Dacia" to run in long zigzags towards Tenerife, sounding about every ten miles, or as circumstances directed, while the "International" made shorter and closer zigzags nearer to the African coast. Map A gives the tracks of the two ships. We will first follow the "Dacia" in her work.

While she was running her second zigzag to seaward, it was noticed that were this course continued a few miles it would cut the old soundings between Lisbon and Madeira at a point where there was a shoaling of from 2,400 to 1,800 fathoms. This shoaling seemed suspicious, and, believing in the possible existence of a bank somewhere near, the vessel's course was continued.

The last sounding on the original line gave 2,400 fathoms, but on running 50 miles farther west only 485 fathoms were found. The whole of one day was spent in exploring the bank thus discovered, and I will quote a description of it from a paper read before the Royal Society of Edinburgh in 1885, by Mr. J. Y. Buchanan, on "Oceanic Shoals discovered by the s.s. 'Dacia' in 1883." Mr. Buchanan says:—"The discovery of this bank, or 'coral patch,' may fairly be claimed as a success in marine diagnosis. The shoalest water found on it was 435 fathoms, in lat.  $34^{\circ} 57' N.$ , long.  $11^{\circ} 57' W.$ , and the depth ranged up to 600 fathoms. The shallow water extends for a distance of 6 miles in an east and west direction, and about  $3\frac{1}{4}$  miles in one from north to south. On the western edge it seemed to fall away precipitously from 550 to about 850 fathoms, when the slope became gentle, and the bottom changed from hard coral to soft ooze. In one sounding on this ledge the sinker distinctly struck bottom in 550 fathoms, tumbled over and continued to sink, struck in 620 fathoms, again tumbled over, and finally found a resting place in 835 fathoms. When it came up it had a large brownish-black streak, where it had evidently struck obliquely on manganese rock. This was a very remarkable sounding, and quite undoubted." The coral subsequently brought up on swabs was examined, and found to be growing luxuriantly.

Continuing her work, the "Dacia," after another zigzag towards Mogador, steered for the Seine Bank, which is the bank I referred to as having been discovered during the laying of the Lisbon-Madeira cable. The shoalest water found by the "Seine" was 100 fathoms. This, and another sounding of 118 fathoms, 12 miles away, indicated the bank. The "Dacia's" first sounding struck the bank in 118 fathoms, but a later sounding gave only 89 fathoms. Buoys were put down, and a survey of the bank made. This mode of procedure is, in my opinion, the only way to get good results, and should always be followed. The distance between the buoys may be measured by running the ship between them, or by paying out wire from one to the other, as was done by the "Faraday" when surveying the Faraday Hills. The relative positions of the soundings with the buoys

may be determined by ordinary triangulation, and as in carrying out this work two or three days will generally be occupied, the latitude and longitude of the locality can be accurately determined by several observations. A profile of this bank, from N. to S., is given on Sheet 1.

After leaving the Seine Bank, the "Dacia" made another discovery, which Mr. Buchanan has described in the following terms:—"The day after leaving the Seine Bank the value of "marine diagnosis was again vindicated. When about 170 miles "south of the bank, a sounding gave 1,189 fathoms with hard "bottom, where at least 1,800 fathoms were looked for. Another "bank was immediately suspected. Three miles farther, on the "same course, 1,386 fathoms were found. If a bank existed it "had, therefore, been passed over. The course was immediately "reversed, and after steaming 7 miles back a sounding gave "810 fathoms; 3 miles farther back 414 fathoms were found, "and 2 miles farther 66 fathoms. Half a mile beyond this "sounding 230 fathoms were found. The ship was again turned "round and steered to the southward for about a mile and a half, "when a buoy with lights was put over in 175 fathoms, and, as "it was already past midnight, the ship lay by it till daylight."

As this bank lay unpleasantly near to the proposed line of cable, two days were spent in determining its limits. It has been called the "Dacia Bank" by the Hydrographic Office of the Admiralty. A profile, from E. to W., is given on Sheet 1.

On trying to raise the moorings of the buoy put down in 175 fathoms, with but little spare rope, they parted at 75 fathoms from the bottom, and were found to have been nearly chafed through at that place. This clearly showed that the bank must rise almost precipitously, and that there exists a wall of about 450 feet in height.

The remarkable experience of the "Dacia" shows that a single deep sounding does not prove the non-existence of a bank in its vicinity. It also shows the necessity of taking soundings at short intervals, and of paying attention to the slightest variation apparent in neighbouring soundings.

The "International," working nearer to the African coast, did

not make any such startling discoveries as the "Dacia," but, nevertheless, found one coral patch and four other shoal spots, which, as they were off the actual line of cable, were not thoroughly explored. There is little doubt that shoaler water can be found in their neighbourhood than is indicated on present charts.

Between Cadiz and Tenerife the total number of soundings taken was 552. In spite of this large number a bank in the immediate line of the cable was missed, and not discovered till during the laying.

While the "Dacia" was paying out cable, the movements of the strophometer, which registers the speed of the paying-out drum, and the behaviour of the dynamometer in indicating the strain on the cable, showed signs of an irregular bottom. Almost at the same moment the "International," which was ahead sounding, was observed to fire a rocket. Shoal water being suspected, the "Dacia" was put full speed astern and an excess of cable was paid out. On getting within signalling distance the "International" reported that she had just sounded in 120 fathoms, thus proving that the precautions taken had not been superfluous. A profile of this bank, shown on Sheet 1, was surveyed on a later expedition by the s.s. "Silvertown," and the shallowest sounding is 84 fathoms. The bank so discovered was called the "Concepcion Bank" in honour of the Spanish frigate which was accompanying the expedition.

Nothing but the prompt way in which the situation was grasped by the engineer in charge of the "Dacia's" deck, saved the cable from an accident similar to that which happened to the s.s. "Seine" when laying the Lisbon-Madeira section of the Brazilian Submarine Telegraph Company's cables. The "International" was not far enough ahead to enable the "Dacia" to avoid this bank, but the measures taken were such as to render it almost certain that the cable will last for a considerable time. Nevertheless, this point is always considered a weak spot in the line; and, should any repairs become necessary in this neighbourhood the course of the cable will, in all probability, be diverted so as to avoid, if possible, this submarine mountain.



On the arrival of the ships at the Canaries they proceeded to sound between the islands, and during the whole expedition 684 soundings were taken for a length overground, in connecting the four stations, of 970 miles.

Although this number is greatly in excess of what is usually considered sufficient, yet a still more thorough investigation would have given greater satisfaction. Sheet 1 shows a profile of the Santa Cruz de la Palma and Garachico (Tenerife) section. The bottom in this neighbourhood is very rocky, and the inclines off La Palma are very steep, as will be noticed. I have extended this profile at each end to the highest point in each island. The Pico de Teyde of Tenerife is 12,180 feet high, and the Pico de Muchachos, in the island of La Palma, is 7,690 feet high. The distances of these peaks from the sea are considerable, and a line connecting the tops of each with the shore does not show a very steep incline. You will readily believe that this line does not in the least represent the outline of the west side of Tenerife, and yet we are generally asked to believe that the outline of the sea bottom in this neighbourhood, and in others exactly comparable with it, can be plotted down from soundings taken 20 or 30 miles apart. There is no reason why the part of Tenerife below the water should be less rugged than that above.

Probably there exists no bottom in the world more unfavourable to cables than that in the neighbourhood of these Canary Islands, and it is exceedingly satisfactory to find that all the care taken in this survey has not been wasted, as the knowledge gained has assured the intelligent laying of the submarine cables which connect these islands together and also join them to the European Continent.

Two other profiles seawards from Tenerife—one off Roxa Point and the other off Teno Point—are also shown.

My object in going so far into this matter has been to impress upon the minds of submarine cable owners the risks they run in allowing their cables to be laid over sea bottoms imperfectly surveyed.

I have purposely avoided giving any details of the way in which the work was carried out, as I hope to describe this fully

afterwards, and to give particulars of the different machines, sounding tubes, thermometers, &c., used.

On another voyage, when returning from a cable-laying expedition in the Brazils, the "Dacia" took 43 soundings, chiefly between Senegal and Cadiz.

In 1884, previous to laying the St. Vincent-St. Jago cable, 46 soundings were taken by the s.s. "International"—an average of about one to every four miles of cable course. After laying this cable she went to Dakar, on the West African coast, and began the survey for the Tenerife-Senegal cable, sounding towards Tenerife. The "Silvertown," meantime, had come out to the islands with the Senegal cable, and also with that to be laid between Lanzarote and Gran Canaria.

One of the results of the survey before laying this last section was the discovery that the reef off Jandia Point, on the S.W. extremity of the island of Fuerteventura, instead of running out 3·8 miles, as shown on the then most recent Admiralty charts, extends to 13 miles. As the cable had to be taken round this, a considerable increase had to be made in the length of the section.

The survey for the Senegal cable, in which the same way of procedure was followed as in that for the Canary Islands cables, showed that the bottom there is thoroughly satisfactory; and, although during the laying of the cable the grapnel was down, no difficulty was met with, as, out of five drags, the cable was hooked four times in 1,600 fathoms, and was brought up to the surface and spliced in the third working day.

The total number of soundings taken between Tenerife and Senegal was 230, over a course of about 800 miles.

The "International" acted as consort to the "Silvertown" during the laying of the Senegal cable, and, on the latter leaving for England, proceeded towards Sierra Leone, sounding and examining landing places. She took 40 soundings in all.

When returning from the West Indies, at the beginning of 1885, the "Dacia" took 60 soundings, chiefly in the old Bahama channel, and some striking irregularities were discovered in that region of coral cays.



The first object of the expedition was to determine the position of the "Buccaneer" in the Atlantic, and to ascertain whether it was possible to lay a cable from the coast of Africa to the coast of America.

In the first place, it was necessary to determine the position of the "Buccaneer" in the Atlantic, and to ascertain whether it was possible to lay a cable from the coast of Africa to the coast of America.

When this expedition started, the "Buccaneer" carried on board a small party of men, and a small amount of provisions. The "Buccaneer" was commanded by Mr. J. Y. Buchanan, and the expedition was under the control of Mr. J. Y. Buchanan. The "Buccaneer" was a small steamer of 460 tons, and she carried two sounding machines and a thorough equipment of the instruments and gear necessary for sounding.

All along the west coast of Africa there is a considerable stretch of shallow water—that is to say, under 100 fathoms; but I need not remind you that ground of this kind is very unfavorable for cables, and nowhere more so than on this coast, where there is a great deal of rock, where the currents are very strong, and where so many mouths of large rivers have to be crossed. The object of the "Buccaneer's" survey was, then, to find whether the incline from the 100-fathom line to deep water is one on which a cable could safely be laid, choosing, if possible, a depth of 200 fathoms.

The "Buccaneer" is a small steamer of 460 tons. Her speed is about 12 knots, and she carries two sounding machines and a thorough equipment of the instruments and gear necessary for sounding.

The expedition was under the control of Mr. J. Y. Buchanan, assisted by Mr. J. Rattray as naturalist. Both these gentlemen volunteered to accompany the expedition to a part of the Atlantic hitherto unexplored, in the hope of obtaining information which would supplement the results obtained during the "Challenger" Expedition. I am glad to say that this hope was

The total number of persons on board was nine officers and 47 men. Any scientific work not interfering too much with the primary object of the expedition was to be carried out, and a great deal was done in this direction. It hardly, however, comes within the scope of my paper, and time will not allow me to do any more than briefly mention the actual sounding work.

The way of procedure adopted on the "Buccaneer" for obtaining her object was to run out from the 100-fathom line, sounding at such intervals as would not give a greater increase in depth than 200 fathoms between two consecutive soundings, until a suitable incline was met or a level bottom reached. A series of courses was therefore run from shore at various points on the coast where changes in the regular features of the bottom might be expected, and the profiles so obtained were connected by a line of soundings in fairly deep water.

The general feature of the profiles along the Guinea coast is the very steep incline, especially on the east side of all the chief points; this is well marked in the profile from Cape Three Points, shown on Sheet 1. Within the influence of the Niger and Congo, and other rivers, the slopes are abnormally gentle. A profile is shown, on Sheet 1, from Gaboon, and you will contrast this with the one just below it.

Between Sierra Leone and Cape Three Points eight profiles were taken, consisting of 128 soundings. A very curious gully off Bassam, on the Ivory Coast, was visited and explored. This gully is called on the Admiralty charts the "Bottomless Pit." The "Buccaneer's" second sounding struck it, and five profiles were taken parallel with the coast line. Map B shows the soundings and contour lines of this interesting gully, which, perhaps, at some distant period was the mouth of the River Akba.

After leaving Porto Novo a fairly straight line was run to S. Thomé. At this island some days were spent in taking profiles, some of which are on Sheet 1. The reproduced profile which extends out to Gaboon is that over which the cables were subsequently laid, and it will be noticed that the slopes on this are not so steep as on some others; but all these slopes, as well

For the St. Jago-Bathurst section of the African Direct Telegraph Company's cables the "Silvertown" took 53 soundings; and the bottom seeming everything that could be wished, this was considered enough.

In her subsequent work between Bathurst and the Isles do Los she was accompanied by the s.s. "Buccaneer;" and this brings me to speak of the latter vessel's recent survey of the west coast of Africa from Sierra Leone to St. Paul de Loanda, a length of coast line of about 3,000 miles.

When this expedition started, the Silvertown Company considered it certain that a cable would shortly be laid to connect the European possessions on the west coast of Africa with the Cape Verd Islands, and there seemed every probability of this company obtaining the contract for a part of the work. As the "Buccaneer" was then on the coast, it was decided that she should make a survey, with the idea that her soundings would be of use under any circumstances.

All along the west coast of Africa there is a considerable stretch of shallow water—that is to say, under 100 fathoms; but I need not remind you that ground of this kind is very unfavourable for cables, and nowhere more so than on this coast, where there is a good deal of rock, where the currents are very strong, and where so many mouths of large rivers have to be crossed. The object of the "Buccaneer's" survey was, then, to find whether the incline from the 100-fathom line to deep water is one on which a cable could safely be laid, choosing, if possible, a depth of 500 fathoms.

The "Buccaneer" is a small steamer of 460 tons. Her speed is about 12 knots, and she carries two sounding machines and a thorough equipment of the instruments and gear necessary for sounding.

The expedition was under the control of Mr. J. Y. Buchanan, assisted by Mr. J. Rattray as naturalist. Both these gentlemen volunteered to accompany the expedition to a part of the Atlantic hitherto unexplored, in the hope of obtaining information which would supplement the results obtained during the "Challenger" Expedition. I am glad to say that this hope was realised.

The total number of persons on board was nine officers and 47 men. Any scientific work not interfering too much with the primary object of the expedition was to be carried out, and a great deal was done in this direction. It hardly, however, comes within the scope of my paper, and time will not allow me to do any more than briefly mention the actual sounding work.

The way of procedure adopted on the "Buccaneer" for obtaining her object was to run out from the 100-fathom line, sounding at such intervals as would not give a greater increase in depth than 200 fathoms between two consecutive soundings, until a suitable incline was met or a level bottom reached. A series of courses was therefore run from shore at various points on the coast where changes in the regular features of the bottom might be expected, and the profiles so obtained were connected by a line of soundings in fairly deep water.

The general feature of the profiles along the Guinea coast is the very steep incline, especially on the east side of all the chief points; this is well marked in the profile from Cape Three Points, shown on Sheet 1. Within the influence of the Niger and Congo, and other rivers, the slopes are abnormally gentle. A profile is shown, on Sheet 1, from Gaboon, and you will contrast this with the one just below it.

Between Sierra Leone and Cape Three Points eight profiles were taken, consisting of 128 soundings. A very curious gully off Bassam, on the Ivory Coast, was visited and explored. This gully is called on the Admiralty charts the "Bottomless Pit." The "Buccaneer's" second sounding struck it, and five profiles were taken parallel with the coast line. Map B shows the soundings and contour lines of this interesting gully, which, perhaps, at some distant period was the mouth of the River Akba.

After leaving Porto Novo a fairly straight line was run to S. Thomé. At this island some days were spent in taking profiles, some of which are on Sheet 1. The reproduced profile which extends out to Gaboon is that over which the cables were subsequently laid, and it will be noticed that the slopes on this are not so steep as on some others; but all these slopes, as well

as those of Principe, are of the steep character usually met with in the neighbourhood of volcanic islands.

After sounding to Principe, the "Buccaneer" ran to Gaboon, and on her way back to S. Thomé took the very complete profile shown on Sheet 1.

Continuing the work south, a line was run to the Congo, and here some valuable work was done. The current in this immense river is very strong, and, while sounding, the ship's engines had to be kept half-speed ahead. With hemp sounding line it would have been impossible to get bottom. Some interesting profiles were taken across the mouth of the river. One taken north of Shark Point showed that, well in the river, we have an enormous channel 2 miles wide and 242 fathoms deep. These soundings in the mouth of the Congo alone would furnish ample material for a separate paper.

At Loanda the survey on the line of cable ended, and the ship returned home *via* Ascension, sounding and doing other scientific work during the passage. The total number of soundings for the whole voyage was 411.

Whilst the repairs were being effected by the s.s. "International," in 1886, to one of the Havana-Key West cables, 9 soundings were taken in order to supplement those made on former expeditions to this part of the world, and 56 for the purpose of finding a route, for some short cables, along the Cuban coast. During the latter work a striking case occurred, showing clearly the value of careful sounding. In selecting a landing place at Havana for the coast cable, the soundings off the spot chosen—which, by the way, was the only one available owing to the nature of the coast—showed a little way from the shore a sudden drop into a deep gully. To the westward soundings proved the continuation of this gully, while to the eastward the bottom, on the other hand, shoaled abruptly with almost a vertical cliff. Instead of choosing between what appeared to be two necessary evils, the sounding machine was again set to work, with a most happy result. Between the two bad profiles a gradual slope was found running out into deep water—not very broad, but broad enough for the

cable to safely rest on, if carefully laid; thus, with a little extra trouble, a good landing place was secured. The two profiles referred to are shown at the bottom of Sheet 1, and marked "San Lazaro Landing." The dotted line is the profile running straight out from shore into the gully, and the *line* that on which the cable was laid. A profile is also shown from Cojimar which is remarkable for its steepness.

In the early part of this year the "Buccaneer," while repairing a cable off the mouth of the Congo, spent some considerable time taking further soundings in that neighbourhood, and on her way home sounded between the islands of Anno Bom, S. Thomé, and Fernando Po. During the whole voyage 183 soundings were taken. Map C gives the apparent form of the Congo cañon or gully after these latter soundings. The positions of the 202 soundings are shown in dots of half a mile diameter, which will give some idea of the distances between the soundings.

This is the last survey I have to mention. I have not in any way attempted to give you a history of deep-sea sounding. Soundings which are merely useful as indicating depths of water in certain parts I have almost entirely neglected, dealing only with those which have been taken with the object of ascertaining the configuration of the ocean bed in certain localities; and I need scarcely add that this work has been done almost exclusively by telegraph ships.

I will now call your attention to the instruments and machines used in sounding, beginning with the sinkers.

I have already mentioned that probably the first reliable deep-sea soundings ever taken were by Sir James Ross in 1818. To Ross is also due the invention of the so-called "deep-sea clamm," by means of which specimens of the bottom were for the first time brought up from great depths in any quantity. This apparatus was in the form of a pair of spoon forceps, kept apart while descending, but on striking bottom closed by a falling weight. A fair sample of the bottom was generally brought up between the spoons, sometimes as much as 6 lbs. in weight. Two separate casts were usually made—one to ascertain the depth,



and the other to recover bottom. The line used was a 2½-inch whale line of the best hemp.

The small weight recommended by Ross as a sinker makes one sceptical as to the value of his soundings, and, indeed, many of them have since been removed from our charts.

The very oldest form of sinker was, of course, the ordinary deep-sea lead, armed at the bottom with tallow; and another old model was the "lead cup," a sketch of which is shown in Fig. 1. It is a very useful arrangement for small depths.

The first great improvement in sounding was the invention of a detaching sinker by Lieut. Brooke, of the United States Navy. His invention, shown in Fig. 2, enabled a very heavy weight to be used as a sinker, which, on striking bottom, was detached and left behind when the tube was drawn up. The arrangement is so simple that it hardly needs explanation. On the tube B striking bottom the lines A A slack, and allow the arms C C to be pulled down by the weight D. When these arms have reached the positions indicated in dotted lines, the slings supporting the weight have slipped off, and the tube can be brought up alone. This principle is the same as that applied by Hooke two centuries ago, and of which mention was made in the early part of this paper.

Brooke's tube was improved on by Commander Dayman, R.N., and used successfully by him on the "Cyclops" in 1857. A modification of it has recently been devised by Mr. Benest, of the Silvertown Company, and is found to answer well. It is made in a cheap form with gas piping, and is useful when a length of wire not altogether to be depended on has to be used, and it is not advisable to risk an expensive instrument. One of these tubes is on the table.

The American idea of a thin line and heavy weight, as used on the "Dolphin" in 1853, was undoubtedly a step in the right direction. It is quite possible to take a fairly accurate sounding in 500 or 600 fathoms with small twine and a 30-lb. sinker, and this system might very well be used on telegraph ships while laying a cable, when it is of importance to have as little delay as possible. No attempt at recovery would be successful, and the sounding



is only useful as indicating the depth. In an ordinary survey, where soundings are being taken close together, and where the character of the bottom is pretty well known, every alternate cast might be taken with twine.

An altogether new system of sounding was adopted on the U.S.S. "Arctic" in 1857. On this occasion the depth was found by Massey's machine. This apparatus is a fairly good one, and has been used pretty recently. Its principle is well known to all sailors and telegraph-engineers. It registers the vertical descent only by indicating the revolutions of the horizontal helix attached to the sinking weight, and the depth is quite independent of the amount of line paid out. On the U.S.S. "Yantic," in 1870, when 134 soundings were taken in this way, the length of line paid out was sometimes as much as double the vertical depth.

Passing over the "Bulldog" and "Fitzgerald" machines, which were soon superseded, we come to the "Hydra." This was invented by a blacksmith on board H.M.S. "Hydra," and is a very good machine. It is shown in Fig. 3. The weights are supported by a line passing over a stud (D). On striking bottom the sounding line slacks and allows the spring C to return to its natural position, shown in dotted line, and in so doing to push the sling off the stud D. The adjustment of the spring is of great importance, as if it is too strong the weights are detached on the least slacking of the line. This machine was used on the "Challenger."

The "Baillie" machine (Fig. 4) is, probably an improvement. The brass tube A has its upper edges bevelled, and in it slides up and down an iron weight, shown in dotted lines, having shoulders on its upper edges. The sinker is held up by a sling passing over these shoulders, which, when the weight is suspended, project above the tube A. When bottom is struck and the strain is taken off, the wire naturally slackens; the plunger H then sinks into the tube A, and the sling supporting the sinker slips off the rounded edges of the tube A, thus becoming detached.

Both the "Baillie" and the "Hydra" machines have butterfly

valves inside, so that they seldom fail to bring up a good sample of bottom.

The tube used on the "Blake" was what is known as the "Sigabee-Belknap." A section of this is shown in Fig. 5. The sinker is suspended by a sling passed over the hook B. On the tube striking bottom, the lever A (Fig. 6) falls, and allows the hook B to tumble; the sling then slips off the hook, and the weight is released. The small spring C helps in this. Fig. 7 shows the weight slipped and the hook kept back by the spring C. This detaching gear, which is a decided improvement on any of the then existing forms, has seldom been known to fail, but is liable to do so when the bottom is very soft ooze, as then the whole tube buries itself and the lever A is prevented from falling. Another objection is that the weight is not always kept on long enough to force the tube well into the bottom. To remedy this defect, and to suit the "Silvertown" method of sounding, a tube was arranged which only releases the sinker on the wire being hauled taut in picking up.

A section of this instrument is shown in Fig. 8, and details of the detaching gear in Figs. 9 and 10.

Fig. 9 shows the detaching gear ready to lower away, the weight being carried by a soft iron wire passing over the hook A. The lower edge of the lever B is a knife edge, and works against the side of the hook A with a shearing action. In the position shown in Fig. 9 this knife is prevented from cutting by the tumbler D; but on striking bottom the wire slackening allows the lever B to fall, freeing the tumbler D, and the spring C capsizes this tumbler into the position shown in Fig. 10. Then, on the wire being hauled taut from on board, the knife is free to cut the sling and release the weight. This arrangement, although possessing excellent qualities, and to a great extent realising the objects for which it was designed, has some weak points. It is certainly an advantage to have a strain on the wire before the sinker is released, because this enables the wire to be hauled taut and got vertical; but the jerk which is almost necessary to make the knife cut the sling is dangerous, and may carry away the wire. Again, the knife may be blunt, or out of adjust-

ment, or the wire sling rather hard for cutting, and the weight is not detached. Besides these disadvantages, the one I pointed out in the "Sigsbee-Belknap" is also present: the knife cannot be released unless the lever is free to fall, and sometimes it is embedded in mud and cannot.

The "Sigsbee-Belknap" tube has a spring valve at the bottom, very nicely adjusted, and seldom fails to bring up a specimen of bottom. The "Silvertown" tube, besides this, has three small tubes, placed side by side, and the specimen of the bottom is held in these, while the bottom water alone goes into the main tube.

To maintain the essential points of the cutter arrangement shown in Figs. 8, 9, and 10, and at the same time to get rid of some of its bad points, I have recently arranged a detacher. The general arrangement is shown in Fig. 12. The chief difference is in the slipping arrangement, details of which are shown in Fig. 11, A, B, C. There are three positions. First, when paying out wire (A, Fig. 11), the wire sling supporting the sinker is supported on the hook H, which is kept from capsizing by the stud D. On striking bottom the spring S pulls the stud D down to the position shown in B, Fig. 11; but the cant of the hook H is not enough to allow the sling to slip off. When, however, the wire is hauled on board, the stud D is pulled up into the first position again, and in doing so capsizes the tumbler completely, and the sling slips off the hook. There is also a difference in the water tube, which is made to hold 1 litre, this being the smallest amount of any use to a chemist.

When no specimen of bottom water is required, this large tube can be replaced by a simple pipe, or a set of three pipes, all of which are made interchangeable.

The arrangement, however, has never been tried, and I will not take up any more time with it.

It is sometimes hard to tell whether, in the case of the tube bringing up no specimen, this is due to rock, hard sand, or extremely soft ooze. On the "Buccaneer" all old notches and dents in the bottom of the tube were carefully filed down after every sounding, and if fresh ones appeared it was considered fair to conclude that rock had been struck.

The best way to find out whether a bottom is rocky or not is to tow a grapnel over the ground, as has been done by Messrs. Siemens Bros.

I must now go back to 1872, which is the year in which deep-sea sounding was revolutionised by Sir W. Thomson advocating the use of pianoforte wire for the purpose. We have already seen that as early as 1838 iron wire was tried in the United States Navy, and ten years later in our own. Both these trials resulted in failures. Sir W. Thomson's very first sounding was a success, and the way in which it was taken differed from previous attempts in little else but a form of brake which he arranged. I said this sounding was a success, but perhaps I should have said half of it was. The machine used to recover the wire broke down completely. Sir W. Thomson's method was taken up by the Silvertown Company, and at the end of 1872 they took the soundings required for the laying of the Lizard-Bilbao cable with wire, as already mentioned, using a machine of their own for its recovery. These soundings were successful. The United States Navy, however, were no slower in adopting the new method, and in their hands Sir W. Thomson's machine underwent much improvement. In August, 1874, a complete set of wire sounding machinery was fitted to the "Blake." The "Tuscarora" had already done splendid work with a similar machine.

In 1873 the "Challenger" Expedition sailed, and 10,000 fathoms of wire were shipped on board. No use, however, was made of it, although it was found impossible to get bottom in the three-knot current of the Gulf Stream by the hemp lines employed.

The "Challenger" Expedition was not a sounding expedition only, and very valuable instruments were frequently attached to the sounding lines. Under these circumstances, and bearing in mind that time was of no importance, as other scientific work was always going on during a sounding, there is little doubt that the lines employed were on the whole the best suited for the general purposes of the expedition.

No one has now any doubt as to the superiority of steel over

hemp for purely sounding purposes. A glance at the relative sizes of hemp and wire sounding lines is almost enough, and a very brief statement of the advantages of wire will suffice.

Its first great advantage is its high breaking strain—some 18 times that of the best hemp, bulk for bulk. Its next is its smooth surface and small superficial area, offering very little resistance to water. In most depths the striking of the sinker on bottom is made at once evident by the sudden slackening of the wire. The method of sounding introduced by Sir W. Thomson was to balance the length of wire out by weights on a brake strap, and thus ensure the stopping of the drum the moment the weight overboard is reduced by the sinker coming to rest on the bottom. Fig. 13 shows a very pretty way of increasing the weight on the brake as the wire runs out. Every turn of the drum moves a weight sliding on the lever, to which is attached the brake strap, a little farther out, and consequently increases the brake power, thus automatically carrying out Sir W. Thomson's method. Although this manner of sounding is a very sure one, and perhaps under certain conditions the best, it is not usually employed. It is found, in practice, that if a fair rate of paying out is maintained by means of a sufficiently heavy sinker, and if necessary by diminishing the brake power, there is no difficulty in detecting when bottom is struck. This is observed by the sudden diminishing of the speed at which the wire runs out. With practice it is easy to distinguish between this and any slacking due to movement of the ship.

The following tables show the rate of descent per 100 fathoms during a sounding taken on board the "International" in 1883. I have chosen this as being a fair sample of work.

## "INTERNATIONAL"—SINKER, 50 LBS.

Fathoms.	Time.			Interval.	
	Hour.	Min.	Sec.	Min.	Sec.
0	■	■	45	—	
100	3	34	25	0	40
200	3	35	10	0	45
300	3	35	56	0	46
400	3	37	46	0	50
500	3	38	40	0	54
600	3	39	35	0	55
700	3	40	30	0	55
800	3	41	28	0	58
900	3	42	28	1	0
1,000	3	43	32	1	4
1,100	3	44	34	1	2
1,200	3	45	30	1	2
1,300	3	46	43	1	7
1,400	3	47	55	1	12
1,500	3	49	8	1	13
1,600	3	50	25	1	17
1,700	3	51	40	1	15
1,800	3	52	50	1	10
1,900	3	54	5	1	15
2,000	3	55	20	1	15
2,015	3	55	30	—	

Corrected depth = 2,060.

Hour. Min. Sec.

4 0 30 began picking up.

4 22 40 lead up.

0 48 55 total time of sounding.

The mean rate of sinking of hemp lines, taken from "Challenger" Report, is given, as follows:—

## "CHALLENGER"—SINKER, 448 LBS.

Fathoms.	Time.			Interval.	
	Hour.	Min.	Sec.	Min.	Sec.
500	9	0	0	—	
600	9	1	8	1	8
700	9	2	21	1	13
800	9	■	39	1	18
900	9	5	2	1	23
1,000	9	6	30	1	28
1,100	9	8	3	1	33
1,200	9	9	40	1	37
1,300	9	11	21	1	41
1,400	9	13	■	1	44
1,500	9	14	52	1	47
1,600	9	16	42	1	50
1,700	9	18	34	1	52
1,800	■	20	28	1	54
1,900	9	22	24	1	56
2,000	9	24	22	1	58

You will notice that there is a very considerable difference of time in favour of the wire, although with it the sinker employed was only about a tenth of the weight of the "Challenger's."

It is not in paying out only that the enormous advantage of wire over hemp comes in, but in the recovery. From depths up to 2 miles wire can be wound in with the ship going 8 or 9 knots, and there need, consequently, be no delay on this head; while with hemp lines the recovery of 2,000 fathoms would take about 2½ hours, and could only be effected with the ship stopped.

I have found from a number of soundings, the aggregate depth of which was 144,500 fathoms, that the mean rate of paying out was 99 fathoms per minute, and of the recovery 90 fathoms per minute—the last, however, with the ship stopped, except for the last 200 or 300 fathoms.

I have seen a complete sounding taken in 1,000 fathoms in



18 min. 16 sec., in 1,500 fathoms in 27 min. 15 sec., and in 2,000 fathoms in 37 min. 45 sec.

The sounding which I have given as an example of the average rate of sinking was also a remarkable one, in so far that the sinker did not become detached, and, with this weight on, the wire was recovered at the usual rate without accident.

I now come to the machine used in the recovery of the wire, and, without going back to any old forms, shall simply draw your attention to two patterns, which are about the best. Fig. 14 is an outline of the machine designed by Sigsbee, and used on the "Blake." It is a very perfect machine, and particularly suited for a small and lively ship. Its principal parts are the drum (A) on which is wound the wire, the auxiliary pulley (B) used while heaving in to relieve the drum of the strain, the jockey wheel (C), the swivel pulley (D), the accumulator contained in the tube H, and the brake (E).

The drum is made light, in order to have as little inertia and momentum to overcome as possible. Its circumference is one fathom. An indicator is attached to the axle, which registers the number of revolutions. The auxiliary pulley B is composed of three pulleys—one for the wire, one for the belt going to the drum, and the other for the belt from the driving engine. A crank is attached to the axle, and is used to assist in starting the engine. The jockey wheel C is an ordinary gun-metal one with a V-shaped score, and the wire passes over this both in paying out and reeling in. Its circumference is 3 feet, and an odometer being attached to its axle the amount of wire paid out can be thus obtained. A very important feature in this machine is the accumulator, which is composed of spiral springs contained in two vertical tubes, one of which is shown at H. These springs are connected with the cross-head of the jockey wheel by means of chains passing over the pulley K. The cross-head moves in steel slides, and rises and falls as the weight on the wire varies, indicating on a scale the strain in pounds. On the "Blake" this machine was placed at the side of the vessel, and about midships. As might be expected, it was found that the rolling of the ship *interfered* with the work very much. To remedy this an

ingenious governor was devised by Sigsbee, making use of the rise and fall of the cross-head to regulate the brake power. The swivel pulley D is constructed to allow the wire to be reeled in at any angle when the ship is going ahead.

Figs. 15, 16, and 17 show the construction of the machine used by the Silvertown Company, and give more of its details than I have been able to show in the case of the American machine. It differs from this last chiefly in having the engine—a small rotary one (E)—on the same base as the rest of the machine, and in there being two positions of the drum—one for paying out, and another for reeling in. The drum on its carriage is moved out by the rack and spur-wheel, as shown in Fig. 16, into the position shown in dotted line in Fig. 15, and the wire passes directly overboard without any intermediate pulley. An odometer attached to the drum shaft gives the length out in fathoms, after applying a correction for the varying diameter of the drum on account of the wire being reeled on it. For reeling in, the drum is moved back into the position shown in Fig. 15, and the wire leads on to it after going over the swivel pulley and round the auxiliary pulley. The auxiliary pulley is driven by spurred gearing from the engine, and from it a belt passes up to the drum. This belt is tightened as required by means of a lever and wheel (L, Fig. 15).

It would be invidious to go into the different merits of these two machines. They are designed for different purposes—Sigsbee's for a midship position on a small vessel, and the Silvertown one for a position at the stern of a fair-sized ship. The accumulator in Sigsbee's is certainly a valuable supplement, not only as an accumulator, but also as a strain indicator. On the "Buccaneer" an ordinary spring balance was used for this last purpose, and worked so well that the lifting of the tube out of the mud could be detected. Sigsbee's method of applying the brake is a good one, and the difference in the readings on the two spring balances gives the resistance imposed on the wheel.

The gauge of the wire generally used for sounding is .030 of an inch. The breaking strain is about 230 lbs. Its weight is  $1\frac{1}{4}$  lbs. per 100 fathoms, and cost about 7d. or 8d. per pound.

Drums of wire, when not in use, should be kept in iron

air-tight tanks, filled with a solution of caustic soda, oil, or lime-water.

Passing on to deep-sea deposits, the following table gives the names of the six principal kinds commonly met with:—

#### PRINCIPAL KINDS OF DEPOSITS.

1. Shore deposits.
2. Globigerina ooze.
3. Pteropod ooze.
4. Diatom ooze.
5. Radiolarian ooze.
6. Red, grey, and chocolate clays.

Of these, the shore deposits may be looked on as the worst, chemically speaking, for a cable. In shallow inshore waters submarine cables are exposed to the dangerous effects of decaying animal and vegetable matter, which is sure to be present, more particularly near the mouths of large rivers. The iodine contained in seaweed has been known to rapidly destroy iron. It is a good rule, both mechanically and chemically speaking, never to lay a cable in shallow, if deep water, with a fairly level bottom, can be found anywhere near.

It is extremely difficult to say what action, if any, goes on between the iron of a cable and any of the component parts of deep-sea deposits; nor is it easy to determine what are the component parts of deep-sea deposits which are known to be most dangerous to the armour of a cable. The effects produced are, however, unmistakable, and of not unfrequent occurrence; for, though the specimens obtained on a line of soundings may lead us to believe that the bottom is quite harmless, experience has repeatedly shown that here and there isolated patches exist of some agent very destructive to the sheathing wires.

Globigerina ooze is not found at greater depths than 2,500 fathoms, and pteropod ooze not deeper than 1,500; the reason for this being that the delicate shells of carbonate of lime which compose these cannot withstand the action of the water sufficiently long to sink deeper. This dissolving action of the sea water is

due to the presence of free carbonic acid gas, and also to its alkalinity.

Of all bottoms these oozes are the ones to be preferred. The fact of their being found shows that no currents exist in those parts, and they are so soft that the cable sinks far down into them. The old idea that currents do not exist at any great depths has long since been rejected. Currents may exist at almost any depth. Between the Canary Islands there are strong currents 1,000 fathoms below the surface, and their scouring action may be clearly detected.

In many places in the ocean bed, no doubt, outcrops of veins of minerals exist as they do on land, and where possible these are to be avoided. Between Havana and Key West, in the West Indies, and indeed throughout almost the whole of the West Indies and the Gulf of Mexico, deposits are found which have caused great damage to cables through chemical action. The existence of these deposits was treated on in the *Electrical Review* of 5th August, 1886.

It is on repairs to submarine cables that the most valuable opportunities occur of gaining experience, which are either too seldom taken advantage of, or the results too seldom made public. Broken or damaged cables should be studied chemically on the spot along with the freshly-collected mud. Both mud or ooze and the products of corrosion of the cable undergo great chemical alteration in drying or in being preserved wet under circumstances which differ much from those existing at the bottom of the ocean.

There is another matter which I have to mention, and that is the importance of taking the temperature of the water at the bottom pretty frequently. Besides the great importance of knowing the temperature of every mile of cable after laying, bottom temperatures enable the student to predict with tolerable accuracy the existence of submarine ridges, and aid him materially in arriving at his conclusions as to the configuration of the ocean bed. The form of thermometer generally used is Buchanan's "Miller-Casella."

I need hardly remind you that of just as much importance as

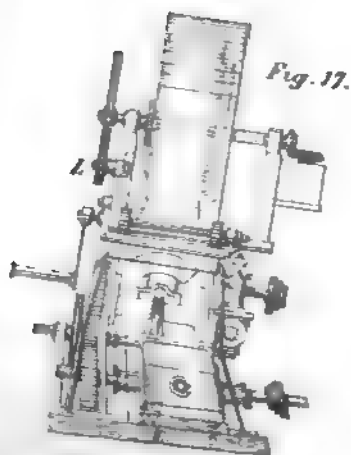
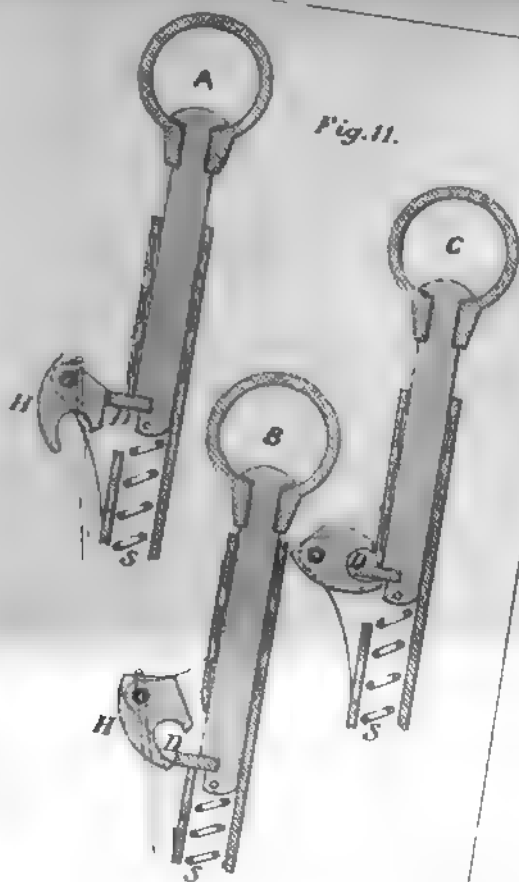


Fig. 16.

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*tical*



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18



there is no doubt that this can best be effected by putting down buoys when possible.

In concluding, I desire to express my best thanks to the Silvertown Company for the opportunities afforded me in preparing the paper to which you have so kindly given a hearing.

The PRESIDENT: I see that Captain Wharton, the Hydrographer to the Admiralty, is present, and I hope he will favour us with any particulars of recent improvements in the matter of soundings which have come under his notice, or have been introduced in his department. The President.

Capt. W. J. L. WHARTON, R.N., F.R.S.: Mr. President and gentlemen,—As one who takes a very great interest in deep-sea sounding in all its phases, I listened with much pleasure to Mr. Stallibrass's paper. I think there is nothing in it that has more interest than the discoveries which have been made of those banks in the Atlantic during the recent surveys made by the Silvertown Company and by the "Seine;" they are very significant to all who have to do with deep-sea sounding, because, as Mr. Buchanan and Mr. Stallibrass have pointed out, they show how very little dependence can be placed on one deep sounding as evidence of non-existence of banks. Single soundings every here and there in the open sea appear close together on a chart; but if the depth of the water is plotted down on paper as a section, you will see that really it does require a very little slope to produce a shoal—it may be even a dangerous shoal—within five or six miles of the straight. This has, I think, however, always been recognised. Sounding is still in its infancy: the world is very large; and, until the submarine telegraph cables demanded it, nobody's attention was very much drawn to the matter; and it really almost seems as if it has taken all this time to recognise the necessity of the soundings being close together for telegraph purposes. It appears that only in very recent expeditions have soundings been taken as closely as we see that they have been done in the sections described to us to-night. I feel a little bit on my defence to-night, and perhaps in the defence of my predecessors in the Hydrographic Office, because, though Mr. Stallibrass did not, I am sure, mean any Capt. Wharton.

Capt.  
W. Barton

reflection, there is no doubt that his remarks sound as if the British Admiralty had been rather behindhand in deep-sea sounding, especially when regarded in contrast with the action of the United States Government. But I would ask you gentlemen to consider what the British Hydrographic Office has to do as compared with the United States. Only a certain amount of work can be done with a certain amount of money; and our ships of commerce continually demand fresh charts, not so much of the deep seas, but of the coasts and of the shores, and it is more than we can do to keep up to those demands. If I were allowed to employ twenty ships instead of eight or nine, as is the case, we still should not have enough. That is one of the reasons why no regular deep-sea sounding expeditions have been fitted out by the British Admiralty. But at the same time the matter has not been altogether lost sight of. Our surveying ships are continually at work all over the world; they are all fitted with sounding apparatus, and they are all frequently getting soundings while going on with their ordinary work, without costing any money or much time, and not interfering with their ordinary avocations. These soundings have been taken here and there, and in the aggregate there have been a very considerable number of them in the past years. I may mention that at this moment one of H.M.'s surveying ships has been sent specially across the Indian Ocean where no soundings have ever been obtained, for the sole purpose of deep-sea sounding. She is going from the Straits of Sunda to Mauritius, and from Mauritius south, and on to Australia. After that she is going to obtain soundings in the Pacific, across that ocean from New Zealand in the direction of the Sandwich Islands. All that will be done in the ordinary course of her duties, so that we have not altogether lost sight of this very important part of our work.

There is another point. We have been rather slow in taking up wire. That Mr. Stallibrass himself has partially explained, inasmuch as he mentioned that, as in the "Challenger," with the delicate instruments that are sent to the bottom, the line has its advantages; and I entirely concur. I think that—especially where you are not continually sounding, and you are not thoroughly

conversant with the wire—you have more chance of losing the latter; at least, that is our experience up to the present time. At the same time wire is gradually coming more into use, and is now adopted in our surveying ships, which are all supplied with it, and are giving us excellent results, as Mr. Stallibrass shows, at the expense of much less time. Nevertheless, when we send the instruments down to the bottom, we prefer to use the line. I have recently been trying, with very great success, galvanised wire. We have used it now for over a year in two ships, and we get the very best results with it; it saves all the trouble of keeping the wire in a preservative solution, and the galvanising is now carried on to such perfection that we have had no difficulty in the way of the galvanising working off and the line rusting.

Capt  
Wharton.

I do not know that I have anything more special to mention, and I think I have detained the meeting long enough.

MR. J. Y. BUCHANAN, F.R.S.: Mr. President,—It has given me very great pleasure indeed to listen to the very luminous paper which I have heard from my former shipmate Mr. Stallibrass to-night. I think it has fixed our attention on most of the things that have to be done in the course of a surveying expedition with the view to the laying of a deep-sea cable. Perhaps the only fault one might find with it is that Mr. Stallibrass has endeavoured to cover rather too much ground, and has, in consequence, dissatisfied us from time to time by passing too rapidly from one interesting subject to another.

Mr  
Buchanan.

With regard to the sounding with wire, to which Captain Wharton has just referred, I think it might have been indicated by Mr. Stallibrass, as the result of our experience in the "Buccaneer," that in order to secure durability it is necessary that the wire should be fingered as little as possible. The machine should be so arranged that the wire runs out over the pulley by which it is going to be brought in again, and when the weight is at the bottom there should be nothing left to be done but to start the engine and heave it up again; and when it is hove up it should be immediately ready for another sounding. In that way a wire, even ungalvanised, will last for a very long time; and there is no objection, if it is in careful hands, to entrust even costly

Mr.  
Buchanan.

instruments to it. I am particularly glad to hear that galvanised wire in the hands of officers of the Royal Navy has given good results, because it undoubtedly simplifies matters enormously. In the "Challenger" hemp line was used exclusively. If we had used wire for physical researches—i.e., for temperature observations, and collecting specimens of water—we should very soon have been in a state of starvation from want of instruments, if we had not starved before for want of wire.

I think that the paper which Mr. Stallibrass has given us points a moral to which it might be well to give some prominence. It is to the assistance which science renders to telegraphy, and telegraphy reciprocally to science. Of course submarine telegraphy is born of science. It was a scientific subject, and in its commencement was worked out purely scientifically; and, after all, science is nothing more than the application of common sense in directions which do not pay; business is the application of common sense in directions which occasionally do pay. But when science has brought forth business there is no reason why it should be thrown over. It is recognised by law that children should support their parents, and the cable-laying companies have not failed in this duty. All of them, in the nature of their business, have had to call in science continually, and have given science a lift where they could; and I think they cannot complain that science has not given them a lift in return.

I think, although I am not a telegraph-engineer, I may say that a telegraph cable should lie on mud. If we find hard ground, we know that there must be something to prevent the accumulation of sediment. Now the only thing which prevents the accumulation of sediment is a current, and one help that telegraph soundings have thus given to geographical science is the indication that tidal currents exist even at very great depths in the open ocean. Another purely scientific result, more especially from the "Dacia's" work, is that coral islands in their development probably all pass through the stage of being calcareous pillars, built up by deep-sea corals, before receiving the tropical reef-building species.

I would call your particular attention to the profile of the

section Tenerife to La Palma, which shows how the general inclination of the land above water is continued comparatively below water; and that is one of the contributions to science afforded by the telegraph laid amongst the Canary Islands, namely, that the same form of ridge and valley continues below water, due to the viscous flow of the volcanic lava; and this necessitates very great care in laying cables.

Mr.  
Buchanan.

The determination of temperature has also been referred to, in a sentence or two, by Mr. Stallibrass, as attempted during two Silvertown expeditions, and many useful results have been obtained. In the West Coast of Africa we found that, owing to the lowness of the temperature, if the cable were laid all down the coast in a depth of not more than 50 fathoms, there would be no more trouble with marine animals than if it were laid in the English Channel; so that although, as we have heard, a shallow-water cable is not advantageous on other grounds, if it should turn out to be so we would have no hesitation in putting it in that water. I do not know that I should take up your time by referring to anything further, because the subject is so interesting that one could, in one's remarks, perhaps emulate the length of the paper.

The PRESIDENT: I suppose, Mr. Buchanan, that on the West Coast of Africa you drop down very soon from shallow water into very deep water? That is what I gather from Wyville Thompson's book.

Mr. BUCHANAN: Off what is known as the West Coast of Africa it goes down very rapidly; at the mouths of the great rivers it runs out for a great distance.

The PRESIDENT: I think Captain Tizard—who, I believe, was in charge of the deep-sea soundings on the "Challenger" Expedition—is here, and we shall be very glad to hear any remarks he will kindly favour us with.

The  
President.

Capt. TIZARD, R.N.: My experience in sounding has been with hemp line only. I have listened with a very great deal of interest to Mr. Stallibrass's paper, and especially to his remarks upon the employment of wire for deep-sea sounding. I have not had an opportunity of using wire, but I quite agree in what he says—that

Capt.  
Tizard.



Capt.  
Tizard.

wire is the thing for the future ; there is no doubt about it. For a sounding line the wire is the thing, but I think for other purposes, such as bringing up heavy instruments from the bottom, the hemp line will still have to be used.

Mr. Br. gl.t.

Mr. CHARLES BRIGHT, jun. : The remarks which have dropped from the last speakers are full of interest and value ; at the same time—if I may be permitted to say so—I think, in the Society's interest, we should do well not to forget the text before us this evening, which is, I believe, deep-sea sounding in connection with submarine telegraphy, rather than the history and utility of deep-sea soundings. As far as the utility and scientific interest of such soundings is concerned, there is, probably, no doubt about that in anybody's mind ; but the question as to what *extent* they are especially warranted in connection with submarine telegraphy appears to me to be another matter altogether.

It is pretty universally acknowledged, I suppose, that the more soundings taken, in the vicinity of the proposed line for a cable, the better ; and, furthermore, the value of a thorough knowledge of the nature as well as of the configuration of the bed is also, I should have thought, undisputed ; but it is always well, from a business point of view at any rate, to consider the other side of the question—in this case that of *time* and *expense*.

Now Mr. Stallibrass has told us that the cost of pianoforte steel sounding wire is 7d. or 8d. per pound ; but he has not alluded, however (probably for lack of time), to the entire cost entailed, roughly speaking, by sending out a suitable ship and staff to carry out the very thorough preliminary survey which he has recommended.

I think it was as early as 1860 (or about the time that the joint committee appointed by Parliament had submarine cables under their consideration) that our Past-President Mr. W. H. Preece, in a paper read before the Institution of Civil Engineers on the "Durability and Maintenance of Submarine Cables in Shallow Water," said that we ought to know just as much about the bottom of the sea as we do about the crust of the earth—or words to that effect for which he was severely taxed by the

the eminent engineer presiding on the occasion. Now, however, *Mr. Bright*, this is pretty well acknowledged, and we try to carry the idea out as far as practicable. I imagine that the scale on which this notion of soundings can be put into practice must necessarily depend upon circumstances, *i.e.*, the length of time and purse available. For example, it is not an altogether unusual occurrence for a cable to be required by a certain date, leaving insufficient time, perhaps, for any preliminary surveying expedition at all; moreover, the cost of manufacture and laying of a cable is often found quite enough without adding that of any extra work, the ultimate advantages of which can never be certain. This should be particularly remembered in connection with the early Atlantic telegraphy referred to, when it must have been sufficiently difficult to get the public to put their money in the project at all, and would have been far greater if the cost of any previous surveying expedition had had to be provided for. I am fully aware that in many cases it has been found that the said cost would have been considerably less than the final expense involved by the subsequent repairs, the necessity of which has been attributed solely to no proper bed having been selected for the cable, or else to an unavoidable irregular bottom not being adequately provided for (owing to ignorance of its existence), as by paying out a sufficiency of slack cable to meet the contingency: nevertheless, I suppose that the element of chance in any case must be an important factor; that is to say, it occurs to me that no matter *how* closely soundings are taken—suppose even they are taken at distances of two miles only (the fact is the more soundings one takes the more one wants to take)—even then there is quite a possibility of the existence of a gulley or bank in between these sounding spots, as indeed proved (by the author's own showing) to be the case with the Concepcion Bank, where a most careful preliminary survey had been made between Spain and the Canary Islands—perhaps the most careful ever made up to that time—and yet it was only at the most critical moment in laying the cable mid-ocean that the said bank was discovered.

Of course the chance of mishaps is considerably *reduced* in proportion to the closeness of the soundings, but, at the same



Mr. Bright. time, the original expense is also *increased* in the same degree, which, I maintain, may or may not be repaid as the case may be; for in the same way there is, perhaps, just as much chance of a moderately *even* bottom. This latter fact does, of course, all the more credit (from one point of view) to those who decide on the long-sighted policy of risking as little as possible by spending more money at the outset. There is rather more probability of an even bottom where clay and ooze are found in a long open stretch of deep water; but (as Mr. Stallibrass says) in the neighbourhood of volcanic islands, on the other hand, the bottom is most likely to be irregular, just as much below as above the surface of the water. I think, therefore, that, without being very thoroughly familiar with the condition of affairs, it would be manifestly unwise to condemn any instances where soundings have not been taken in a comprehensive manner previous to laying a submarine cable.

The Lisbon-Madeira cable has been cited as an example where a more complete preparatory survey would have more than repaid the expense afterwards involved by repairs; but I am sure the author did not mean us to take this one instance into the argument alone without taking into account the many other cables which have been laid and in most cases are having, or have had, fairly long lives, notwithstanding that no such complete survey was effected previous to their deposit. Indeed, by far the largest proportion of cable at present submerged is that which was laid previous to 1879, before which date no adequate number of soundings to be of any practical utility for diagnosing the bottom were ever taken; and yet the majority of these cables are, I believe, in capital working order, without having required any extraordinary number of repairs. I am not mentioning this as an argument in opposition to the necessity of soundings, but merely as a point that should be kept in mind.

I would, moreover, remind the meeting that there is a very marked difference between the circumstances that existed in the early days of submarine telegraphy and those which we at present enjoy, if only from the fact that it was not until 1872 that steel wire was introduced in a practical form by Sir William Thomson

as a substitute for hemp. This, I contend, made a very material *Mr. Bright.* change in the practicability of sounding work for our purposes.

It occurs to me that Mr. Stallibrass might have even strengthened his point—*i.e.*, the value of a survey and sounding expedition being made on a large scale as an introductory to laying a cable—if he had referred, on principle, to the arguments on both sides, and, in some degree, balanced the one against the other, particularly as what arguments there are against him suggest, perhaps, a somewhat short-sighted policy. Moreover, I think we must give pioneers credit for having thoroughly considered the matter under the then existing circumstances.

Such surveys are, of course, not only useful in seeking for a regular bottom, but also for ascertaining its nature, which should, then, largely govern the form of cable to be adopted. In fact, if soundings are to be taken at all, they should, I believe, be taken before commencing proceedings (by the telegraph company, or by their engineers, rather than by the contractors), in order to permit the fullest advantages being made of the discoveries brought to light—in time, possibly, to effect a complete alteration in the plans, if found necessary. Unfortunately, however, those requiring cables, not being sufficiently well versed in the subject in all its aspects, seldom allow enough time for this to be done; indeed, it is often owing to their want of knowledge that they do not sometimes procure a more sure return for their money, in spite of the remonstrances of engineers and contractors, to whom it must be extremely unsatisfactory to be compelled to hurry over such very important work.

With reference to the author's remarks anent the action which takes place between the iron sheathing wires and the component parts of various deep-sea deposits, I am under the impression that an analytical chemist—provided that he is able to examine the specimens immediately on recovery aboard ship—is in a position to determine, with tolerable accuracy, all that has taken place chemically, or to judge whether the bottom in question is, or is not, a destructive one in its character to the armour of an iron-sheathed cable; but it is absolutely essential (as the author says) that such specimens be examined before they have

Mr. Bright. been exposed for any length of time, after which they will entirely alter in character and thus give an altogether inaccurate idea of their nature and effects. An intimate acquaintance with the nature of the bottom is certainly quite as important as a knowledge of its configuration, but is, perhaps, more difficult to obtain, owing to its more sudden variation, as by the small patches (referred to in the paper) in the midst of an otherwise excellent bed (chemically) for a cable.

I have ventured to offer these remarks merely in the hope of their possibly proving of utility as a humble addition, from a business point of view, to Mr. Stallibrass's extremely able and interesting paper.

Mr. E. STALLIBRASS, in reply, said: I should indeed be sorry if in anything I have said in my paper I should have given Captain Wharton the idea for one moment that I intended to reflect in any way upon officers of the British Navy, and especially on those connected with his Department. Anyone who has had to do with telegraph cables knows that we have always had the heartiest support from the Navy. It is rather with the inactivity of the British Government in the direction of submarine research that I find fault. It should be remembered that the work of the Hydrographic Office of the Admiralty is to deal more especially with shallow rather than with deep soundings, and until recent years there has been so much of this to do that it was doubtless impossible for the Department to extend operations into deep water.

I have to thank Mr. Buchanan for the kind way in which he spoke of my paper, and I note what he says about fingering the wire when sounding: I have no doubt it is an important thing to avoid this as much as possible.

With regard to the cost of sounding, mentioned by Mr. Bright, time will not permit of my going fully into the matter, and I will merely remark that I think this is generally very much over-estimated. A ship could leave England, thoroughly survey the route for an Atlantic cable, and be back in about 70 days. The cost of this need not be more than £3,000; and by the side of £700,000 or £800,000, which is somewhere about the cost of an

Atlantic cable, this is nothing tremendous. It would be more <sup>Mr. Stallibrass.</sup> than balanced by the loss of 15 miles of cable during the laying or in subsequent repairs.

I have to thank you for the very kind way you responded to the vote of thanks proposed by the President.

The PRESIDENT : Mr. Stallibrass's paper has afforded us a great <sup>The President.</sup> deal of information. It may seem a very easy thing to take a sounding in blue water, and if a leaden weight or a cannon ball is thrown overboard it would soon reach the bottom; but if a line is attached to it the friction of the water alters the conditions of recovering the weight, so that, as reported by Captain Dayman, of the "Cyclops," "the tar was forced out of the rope in an extraordinary manner, several of the splices started, and the rope was "much stretched." In the earlier soundings, unless the weight employed was very heavy compared with the dimensions of the line, the friction of the water caused the sinking of the weight to be very slow, while the line was exposed to the action of currents, making a series of curves and sometimes not reading the bottom at all, as in the instance referred to of 50,000 feet being run out without proof of bottom.

I now ask the meeting to give a hearty vote of thanks to Mr. Stallibrass for his paper.

The motion was unanimously carried.

The meeting adjourned until November 24th.

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The One Hundred and Seventieth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 24th November, 1887—Sir CHARLES T. BRIGHT, M.I.C.E., President, in the Chair.

The minutes of the previous meeting were read and approved.

The name of one new candidate for election was announced and ordered to be suspended.

Donations to the Library were announced as having been received since the last meeting from the Astronomer-Royal; Messrs. Charles Griffin & Co.; and Professor Silvanus P. Thompson, Member; to whom the thanks of the meeting were duly accorded.

The following paper was then read :—

## ON SOME INSTRUMENTS FOR THE MEASUREMENT OF ELECTRO-MOTIVE FORCE AND ELECTRICAL POWER.

By J. A. FLEMING, M.A., D.Sc. (Member), and C. H. GIMINGHAM.

These instruments do not present any novelty of principle. They differ from that form of instrument generally called an electro-dynamometer in the arrangement of details, but they depend for their operation upon the electro-dynamic action of currents in fixed and movable conductors. Our object has been to furnish an electro-dynamometer which shall be portable, compact, and require in its use no mercury cups, tables of square roots, nor multiplying factor, nor any reference other than to the scale of the instrument, in order that electro-motive force and power may be determined by it at once. Our experience in the use and testing of voltmeters of nearly every kind has been such as to lead us to look doubtfully at any instrument which either contains soft iron, permanent or electro-magnets, or in any way depends for its action on the behaviour of iron when

placed in varying or successively varied magnetic fields. No matter how treated, a piece of soft iron has a "magnetic memory." Unless rapid magnetic reversals, aided by rise of temperature, are brought to bear upon it, it is very difficult to eliminate from it the results of its previous magnetic history and to bring it back into the condition in which its magnetisation is the result, purely and simply, of the present magnetic force brought to bear on it.

We start, therefore, with the principle now adopted by Sir W. Thomson, that in thoroughly satisfactory electric-measuring instruments the magnetisation of iron must not be permitted to play any part in the design. In these instruments we submit to the criticism of the Society, the action is wholly based on the fact that, when conductors conveying electric currents are arranged so that one of them is fixed and the other free to move, the force required to hold the movable conductor in any given position in the field of the fixed conductor is proportional to the product of the strengths of the current flowing in these conductors respectively. The general arrangement of the high-resistance dynamometer or voltmeter is shown in Figures 1 and 2, which show cross

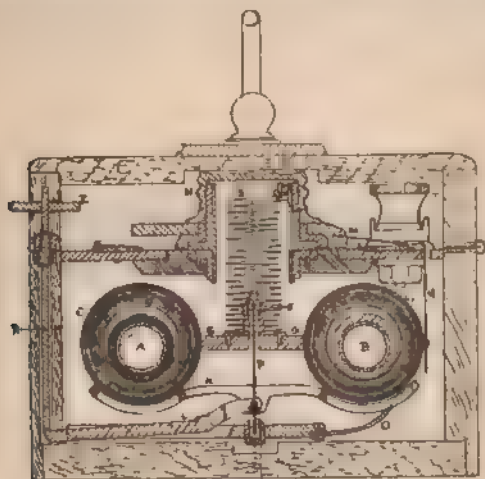


FIG. 1.

sections of the voltmeter. Figure 3 shows a plan of the same. The general design of the instrument is as follows:—A A' B B' (Fig. 4) are two solenoids of insulated wire (German silver)



wound on brass tubes. The solenoids are wound so as to have a magnetic pole in the centre of each, marked N N. The ends of the solenoids are therefore similar poles (S). Two such solenoids

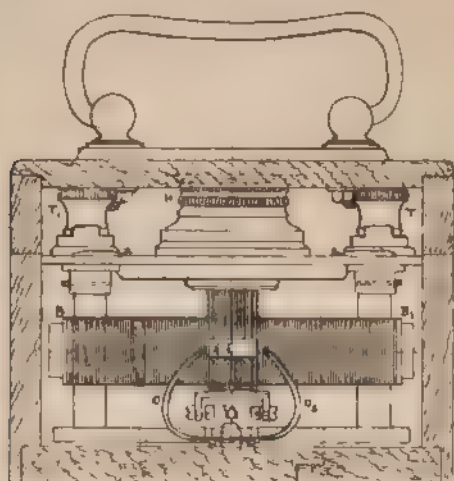


FIG. 2.

are held in a frame (D D', Fig. 3), and fixed so as to be parallel to each other. The general arrangement, free from detail, being shown in the sketches (Figs. 3 and 4).

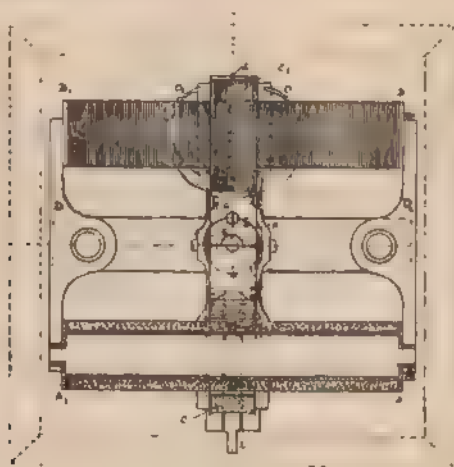


FIG. 3.



These fixed coils are embraced by two annular coils ( $C C^1$ ), which are wound on exceedingly light metal frames and are carried on the ends of an ivory bar ( $G G^1$ , Fig. 1). The resistance of the

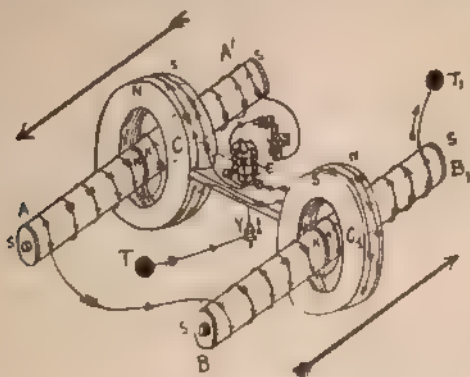


FIG. 4.

fixed coils may be about 1,000 to 1,500 ohms, and the resistance of the annular coils about 500 to 1,000 ohms. This ivory bar carries at its centre a glass-hard steel cup, by which it is supported, so as to move freely, like a compass needle, on a steel iridium-tipped needle ( $P$ ). The coils are very accurately balanced, and, whilst embracing the fixed coils, have yet clearance enough to permit them to swing through a small angle on either side. Their range of movement is, however, limited by two stops ( $O O^1$ , Figs. 2 and 5).

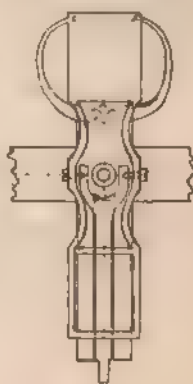


FIG. 5.

To the ivory cross-bar carrying these movable coils is attached a steel spiral chronometer spring, the upper end of which is held in a tubular collar ( $H$ , Fig. 1), capable of revolving stiffly in an aperture in the dial plate. This collar carries on the outside of it a mica-tipped index finger, which is shown separately in Fig. 6. The index finger is capable of being set round on the tubular collar  $H$ , and clamped in any position.

The inside of this tubular collar has an attachment by

which the upper end of the steel spring can be taken up more or less, just as the hair-spring of a watch is set.



FIG. 6.

The electrical connections of the voltmeter are shown in Fig. 4. It will be seen that the current enters from one terminal ( $T'$ ), passes thence to the top of the spring, then down the spring into the movable coils  $C C'$ , from there to the steel cup  $E$ , through the contact-point to the iridium-tipped steel pivot, from that through the fixed coils and so back to the other terminal ( $T$ ). The windings of the movable coil are so arranged that it is astatic, and not influenced by a vertical or horizontal uniform field. This is important in an instrument to be used in the neighbourhood of powerful magnets.

The movable coils carry a light aluminium needle ( $Q$ ), which projects up through a slit in the dial plate, opposite to the zero of the scale.

The whole instrument is contained in a small wooden box, about  $4\frac{1}{2}$  inches square by 3 inches deep. On opening the box a dial plate is seen having a circular scale divided so that the volt readings are obtained without any calculations. The operation of taking a reading is as follows:—The box is first levelled on the table by means of a wooden wedge provided for the purpose. The central hollow boss is first turned until the mica index finger  $M$  (see Fig. 7) is at the zero of the scale. The aluminium index carried by the movable coils (Fig. 8), which projects up through a slit in the dial, should also be opposite the zero. If this is not the case the mica index finger has to be shifted. This is accomplished by slacking up the milled head on the top of the central boss and re-adjusting the index until the mica index finger and the aluminium index of the movable coils both stand opposite to each other at the zero. The current is now passed, and the electro-magnetic action causes the movable annular coils to be shifted along the fixed coils, displacing the aluminium index.

The central boss, carrying the upper end of the spring, is then turned until a torsional force is brought to bear on the movable coils, bringing them back against the electro-magnetic forces into the zero position. It will now be found that the upper end of

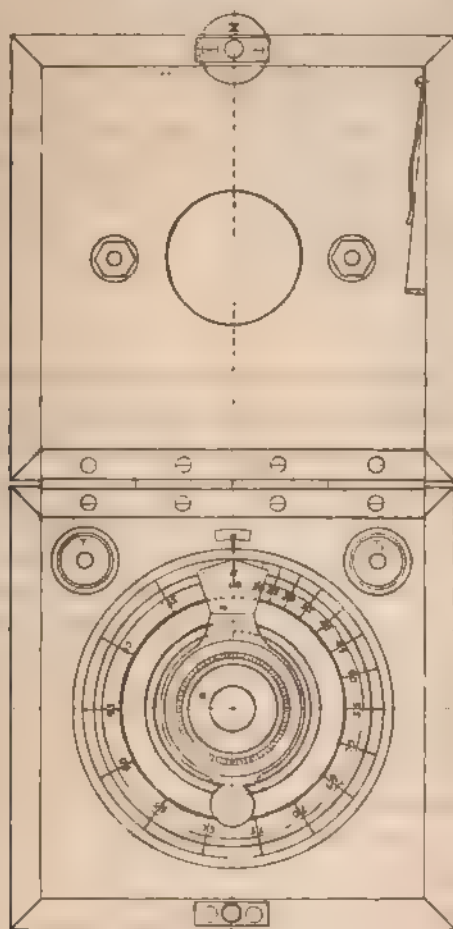


FIG. 7.

the spring will have been twisted through a certain angle, as indicated by the position of the mica index finger. The value of the current passing through the instrument is proportional to the square root of this twist required to bring back the coils into their zero position, and, as the force brought to bear on the

coils is proportional to the angular displacement of the upper end of the spring, it follows that the square root of the angular displacement of the mica index finger required to maintain the

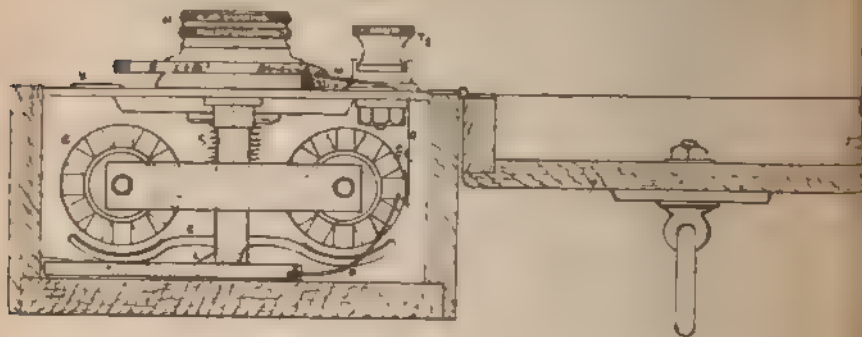


FIG. 8.

movable coils at their zero position is proportional to the strength of the current flowing through the instrument. To avoid any calculations or reference to square-root tables, the dial is graduated in the following manner:—If the instrument is intended to read, say, from 20 to 110 volts, then one complete turn, or  $360^\circ$  displacement of the index, is required to balance the electromagnetic force due to 110 volts on the terminals of the instrument. The angular displacement for  $x$  volts is, then,

$$360 \times x^2 = 18 \times (110)^2 = 605 \times x^2, \text{ measured in degrees, and accordingly the}$$

positions for all volt values from 20 to 110 can be set off directly on the dial (Fig. 9). The index needle is then simply turned until the aluminium index is brought back to zero, and the dial reading gives at once the volts. This form of graduation has the advantage that the scale interval corresponding to one volt is greatest in that part of the scale which will be in each instrument presumably most used.

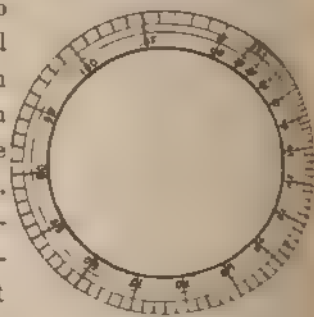


FIG. 9.

In order to render the instrument portable, means are pro-

vided to raise the movable coils off the point P by the action of closing the box. A clutch-bar (K) is so arranged and pivoted, that by means of a pin (R) sliding in a hole in the box a pressure on the upper end of this pin (H, see Fig. 1) raises the coils (C C') and relieves the iridium point of the pressure of the movable coil-bar.

It is not desirable that the movable coils should be taken up and let down suddenly on the pivot, and to make this action deliberate the box lid is opened and closed by a screw. The user is thereby compelled to open the box slowly and let down the coils with a gentle motion on to the pivot P.

The voltmeters are constructed to work over various ranges. One type ranges from 20 to 110 volts. By associating a resistance coil with the voltmeter, the resistance of which is equal to that of the voltmeter, the volt values of all the readings become doubled and the instrument becomes available for use between 40 and 220 volts. In order to give a little margin over and above the 110 and 220 volts, the graduation is continued, according to the square-root law, along an outer circle. A little more than one complete twist of the spring enables a reading to be taken up to 115 or 120 volts on the 110-volt instrument, or to 230 or 240 on the 220-volt instrument. In the 110-volt instrument (construction A), the following are the electrical and mechanical data of the instrument:—The weight of the movable coils complete is in all 20.27 grammes. The number of turns of wire is 960, on both coils together, and the total resistance is 300 ohms. The four fixed coils have each 840 turns of wire, or 3,360 turns in all, and a resistance of 1,200 ohms. The total resistance of the voltmeter is therefore 1,500 ohms. The temperature-resistance variation coefficient has been carefully determined for a mean temperature between 15° and 30° C. for the particular kind of German silver wire used, and it is .0273 per cent. per degree. Accordingly, as far as regards change of temperature as a whole, it requires an alteration of 30° C. to make a change of 1 per cent. in the value of the voltmeter readings. To prevent, as far as possible, heating of the coils, each voltmeter is provided with a key. In using the instrument the first operation after opening should be

to level it carefully, by inserting, if necessary, the wooden wedge provided under the front or back side until the pointer on the movable coils swings freely from side to side.

We have also constructed, on this type, a Wattmeter. In this

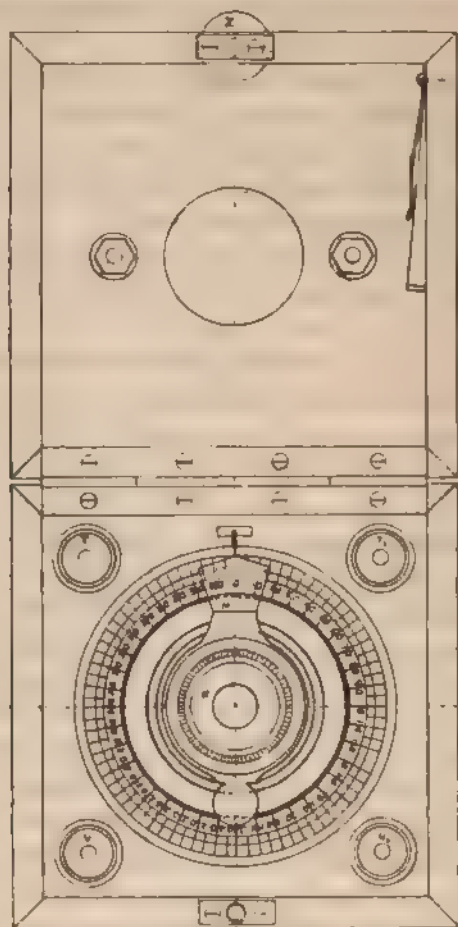


FIG. 10.

case the fixed coils (now called the current coils) are wound with thick wire, and so arranged that the four coils can be joined up in series or in parallel. From a separate pair of terminals the current is conducted into the thin wire coils, which are the movable coils, and constructed exactly as for the voltmeter, only with

a higher resistance (see Figs. 10 and 11). In using this Wattmeter

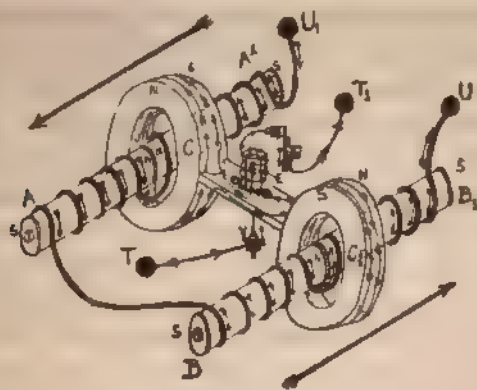


FIG. 11.

for measuring the power taken up, say, in an incandescence lamp, the following method of joining up should be adopted:—

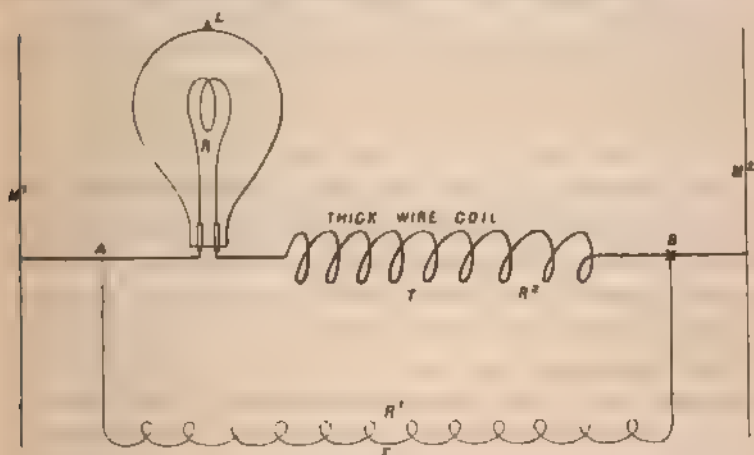


FIG. 12.

Let  $M_1$ ,  $M_2$  be the mains, and let  $L$  be the lamp. The lamp should, as usual, be put in series with the thick wire coils  $T$ . The fine wire coils  $F$  should have their terminals joined, not to the two terminals of the lamp, but to the terminals of the lamp and the thick wire coil as joined in series. The thin wire coil thus measures the potential at the extremities, not of the lamp, but of the lamp and thick wire coil in series. The reason for this is as follows:—Suppose the thick wire coil to have a resistance of  $\cdot 01$



ohm and the thin wire coil to have a resistance of 1,000 ohms, and apply this Wattmeter to measure the power consumed in two lamps, each of 8-candle power and requiring, say, 20 watts—but in one case let the lamp be a 100-volt lamp taking .2 ampère, and in the other a 10-volt lamp taking 2 ampères. If the fine wire coil is joined in parallel with the lamp alone, in the case of the high volt lamp, since the resistance is 500 ohms hot, the lamp will take two-thirds of the whole current flowing through the thick coil—in other words, the current which flows through the thick coil is not that flowing through the lamp, but that through the combined resistance of lamp and fine wire coil. If, however, the same wattmeter is applied to measure the power of the low volt lamp, then, since the hot resistance of this is only 5 ohms, the lamp will get  $\frac{100}{1005}$ ths of the current flowing through the thick coil. It follows from this that whereas the two lamps consume exactly the same power, yet, in the first case, the current in the thick coils, which is that which actuates and is one of the factors in determining the reading of the wattmeter, is half as great again as it should be. It would be found that in the case of the high volt lamp the wattmeter reading would be 30 watts, and in the case of the low volt lamp 20 watts, although both lamps actually take the same power. If, however, the extremities of the voltmeter, or movable or fine wire coils, are joined, as shown in the figure 12, then the current in the thick coil is exactly that through the lamp, the error in the electro-motive force part of the measurement is then only dependent on the fact that the thick wire coil has an appreciable resistance; if this is only .01 ohm, the error made in the electro-motive force part of the measurement is only .1 per cent., even in the case of the low volt lamp, by assuming the potential at the extremities of the lamp and thick coil in series as equal to that at the terminals of the lamp.

Unless the resistance of the current coils is vanishingly small compared with that of the high resistance or movable coils, the above method of joining up the wattmeter does not give the real rate of dissipation of energy in the lamp or other circuit under consideration.

Let us examine the theory of the instrument a little more

closely. Let  $R$  (Figs. 13 and 14) represent the resistance of the thick wire or current coils, and let  $r$  represent the resistance of the fine wire or volt coils, and let  $\rho$  stand for the resistance of the lamp or other circuit in which the dissipation of energy is being measured. Given the ends of these thick and thin wire coils, we may then join them up to the lamp in either of two ways, as shown in Figs. 13 and 14.

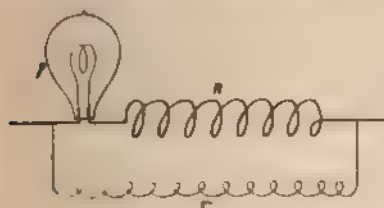


FIG. 13.

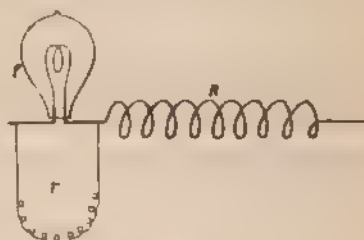


FIG. 14.

What we actually do in the use of a thick and thin wire electro-dynamometer, employed as a wattmeter, is to measure the product of the strengths of the currents in the thick and thin wire coils respectively, by measuring the electro-dynamic attraction between these circuits when held in certain fixed positions. If, however, the circuits are joined up to a lamp as in Fig. 13, although the current in  $R$  is equal to the current through the lamp, yet the current through  $r$  is not proportional to the difference of potentials on either side of the lamp, but greater by an amount which depends on the ratio of  $\rho$  to  $R$ . If  $W$  represents the true watts taken up in the lamp under measurement, and  $W_1$  represents the observed watts by the dynamometer, on the assumption that it has been calibrated for one particular lamp of very high resistance compared with  $R$ , then

$$W_1 = W \frac{\rho + R}{\rho} \quad \dots \quad (i.)$$

for  $W_1$  is made up of two factors—one a number proportional to the current in  $R$ , which is the current through the lamp, and another factor which is proportional to the difference of potentials over all between the outside terminals of lamp and resistance  $R$ .

This last is greater than the potential difference on either side of the lamp, in the ratio of  $R + \rho$  to  $\rho$ .

Next, let the fine wire coils be joined to the terminals of the lamp as in Fig. 14. The current in  $r$  is now proportional to the potential difference on either side of the lamp. The current in the thick wire coils is not, however, identical with that through the lamp, but equal to that through the lamp and fine wire coils together. If  $c$  stand for the current through the lamp, and  $C$  for the current in the thick wire coil  $R$ , then

$$c = \frac{r}{r + \rho} C,$$

and accordingly, if, as above,  $W$  stand for the true watts expended in the lamp, and  $W_2$  for the watts as observed on the dynamometer, we have the relation

$$W_2 = W \frac{r + \rho}{r} \quad \dots \quad \dots \quad (ii.)$$

From equations (i.) and (ii.) we have

$$W_1 \rho = W \rho + W R,$$

$$W_2 r = W r + W \rho;$$

hence, 
$$W^2 \frac{R}{r} = (W_2 - W) (W_1 - W);$$

or, 
$$W^2 - \left( \frac{r}{r + R} \right) (W_2 + W_1) W + \left( \frac{r}{r + R} \right) W_1 W_2 = 0.$$

The solution of this quadratic equation gives  $W$ .

It is not difficult to show that this quadratic equation always has one real positive root, since  $r$  is greater than  $R$ , and  $W_1$  and  $W_2$  are positive quantities. We arrive, therefore, at this result:—If any given wattmeter of this type has been calibrated on a lamp or other circuit of which the resistance is very great compared with the thick wire or current coils, supposing the connections made as in Fig. 13, then the observed readings of the wattmeter, when used with any other lamp or circuit, will not give the true watts expended in that circuit, but give a value which is rather too high, and of which the error will depend on the value of the three resistances,  $R$ ,  $r$ , and  $\rho$ . We can, however, eliminate the error, and obtain a number proportional to the true rate of expenditure of energy in the circuit  $\rho$ , by taking two observations with the high resistance coils joined up respectively as in Figs. 13 and 14. If these readings are called  $W_1$  and  $W_2$ , the insertion of these in the quadratic equation

above will enable us to find a value ( $W$ ) which is proportional to the real watts, and that without knowing the resistance of the lamp being tested.

In the construction of a wattmeter of this type it is evidently important that the current coils should have as low a resistance as it is possible to secure in comparison with that of the movable or voltmeter part. In our wattmeter the fine wire coils are of German silver wire, and have a resistance of 1,000 to 1,200 ohms. The thick wire or current coils are of the highest conductivity copper; and in the instrument designed as a lamp wattmeter, to read direct from 1 to 400 watts, have a resistance of about  $\cdot 1$  or  $\cdot 2$  ohm. It is intended to issue wattmeters of three grades—one reading from 1 to 400, the next from 10 to 4,000, and the highest from 100 to 40,000, and suitable for electro-motive forces of 50 to 250 volts.

In the wattmeter above described the scale is, of course, a scale of equal divisions. In the form of instrument adapted for lamp measurements, the circular scale is divided into 400 divisions, and each division represents 1 watt. By means of a wattmeter and voltmeter of the above description, all the four electric elements of a lamp can be measured at once. Let the wattmeter be joined up as indicated in the above diagram (Fig. 13), and let, in addition, the terminals of the voltmeter be connected to the points A and B, the voltmeter then reads the potential difference of A and B, and the wattmeter reads the consumption of power between A and B. If  $W$  be the reading of the wattmeter and  $V$  that of the voltmeter, the quotient of  $W$  by  $V$  gives the current through the lamp, and the quotient of the square of  $V$  by  $W$  gives the hot resistance of the lamp. We have designed an instrument which shall be a combination of watt- and voltmeter, and enable the two measurements of potential difference at extremities of a circuit and power consumed in that circuit to be simultaneously measured, and therefore to give at once the current in the circuit and the hot resistance. In this instrument the fixed coils would be wound compound, with two coils on each, one thick overlaid by a thin wire winding.

The connections of the fine wire windings on the fixed coils

are so arranged in conjunction with a switch, that when the switch is turned one way the solenoids have, as ordinarily arranged, a N. pole at the centre of each, but when the switch is turned the other way this pole is abolished. Hence, if a current flows through these fine wire windings in series with the movable coils, then, according as the switch is set one way or the other, the current will operate the instrument or not, and that without changing its resistance or affecting the current in the movable coils. Let then the thick wire coils be traversed by a main current, and the thin wire coils by a current derived by connecting their extremities to the ends of the conductor traversed by the current through the thick wire coils. The electro-magnetic force operating to displace the movable part of the instrument will be the resultant of two forces—one due to the product of the strength of the current in the thick coil and the current in the movable coil. This product is proportional to the power consumed in the conductor. Secondly, in addition, there is a force due to the action of the current in the fixed fine wire coils on the current in the movable coils in series with them. This force is proportional to the square of the current strength or to the square of the volts at the ends of the conductor. Hence, if the switch is turned so that both thick and thin wire fixed coils are operative, the electro-magnetic force on the movable coils is proportional to the watts added to the square of the terminal volts. If  $W$  be the power consumed in the conductor, and  $V$  the potential difference at its extremities, then this reading of the instrument will be proportional to  $A W + B V^2$ , when  $A$  and  $B$  are instrumental constants. Next, let the switch be turned so as to render the fine wire coils electro-magnetically inoperative, although not altering the resistance of the total fine wire circuit. In this case the reading is simply proportional to the power consumed in the conductor, or to  $A W$ . We have therefore, by two readings, the power consumed and the terminal volts of the conductor or circuit, and hence, by simple division, the current and hot resistance.

In this method of measuring a current, the movable part of the instrument is not traversed by any but a very small current,

and hence the difficulty generally experienced in getting a large current in and out of a freely-moving circuit has not to be combated. In considering these instruments as permanent standards, the question obviously arises as to the permanence and perfect elasticity of the steel spring. The steel chronometer spring is made of the very best and most carefully-tempered steel. The use in chronometers justifies the statement that, when not strained beyond the limits of elasticity and when not kept in strain for a longer time than necessary to take a reading, the elasticity is perfect. There is only one caution which should be taken in using these instruments. After taking a reading, the central index-hand should be turned back to zero before putting the instrument away. If the voltmeter is closed up and put on one side, with a permanent twist upon the spring, then a slight deformation of the spring may result, which will in time disappear, but not immediately. The elastic modulus which is brought into play in this use of a spiral spring is somewhat similar to that which is operative when a beam is bent. The spiral is not drawn out, but is twisted round and bent slightly into a smaller radius. Under prolonged strain a very slight deformation occurs, which disappears gradually on the removal of the stress, and this deformation is a function both of the stress or strain and the time under which the elastic body is submitted to that stress.

On this point we may quote the experience of Professor W. Kohlrausch, who, in a paper in the *Electrotechnische Zeitschrift* (see *Electrician*, March 25, 1887), gives the result of prolonged experiments on the employment of spiral springs in measuring instruments. The effect of age, judging by observations on a brass spring extending over a period of seven years, may be completely neglected. Continuous and prolonged deformation produces a small amount of permanent set, so that the spring, when released, does not return to its original position, but this does not actually alter the indications of the instrument if the readings are taken from the new zero thus formed. Steel is in this case less affected than German silver. Oft-repeated but intermittent deformation, as exhibited by a spring of 90 convolutions and  $2\frac{1}{2}$  inches long being stretched,



so as to change from a length of 3 inches to 9 inches 200 times per minute for 400 minutes, introduced no sensible alteration into its subsequent indications.

An increase of temperature of  $18^{\circ}$  Fahr. raised the indications of a Siemens torsion galvanometer about one-tenth per cent., so that the reduction in elasticity of the spring is apparently almost equal to the decrease in moment in the magnet. Further experiments on loaded springs confirmed this conclusion, and showed that steel was again to be preferred to German silver. We think, therefore, that there is sufficient evidence to lead us to believe that the selection of a highly-tempered steel chronometer spring as a means of weighing the electro-dynamic attraction of coils traversed by currents will not be found to lead to appreciable errors.

Instruments with the suspension we have here designed and used are not very well suited for marine use, but we are engaged in arranging a mode of suspending the movable coil which will render the instruments independent of any levelling when in actual use.

The  
President.

The PRESIDENT: Now, gentlemen, we have another paper down for this evening, by Professors Ayrton and Perry, which is somewhat allied to the subject dealt with in the paper we have just heard; and the question arises whether you would wish to take the discussion upon the present paper, or to have the second paper read, to be followed by a discussion upon both. I am afraid that taking the second paper before the discussion upon the first may cause members to lose sight of what Dr. Fleming has stated, but I will leave the matter entirely to the meeting. Perhaps some one will make a proposition in the matter; or, as there seems a little hesitation to do so, I will myself propose that the discussion on the first paper take place before the second paper is read.

Mr. Preece.

Mr. W. H. PREECE: There is one reason, Sir, why I think it would be as well that we should take the second paper before the discussion: that is, Captain Cardew is here from Chatham prepared to say something on both papers, and he



wishes to return by a train which he will miss if the proposed course is adopted.

The PRESIDENT: A good many members may wish to do so, The President. but it is unusual to take up two papers at once for discussion.

Professor ADAMS: I should be very glad to second Mr. Professor Adams. Preece's proposition if it requires formally passing. On previous occasions, I think, at meetings of the Society, we have found it very beneficial to take papers on the same subject together and then have a discussion upon them.

Mr. Preece's motion, having been put from the Chair, was carried.

The following paper was then read:—

## PORTABLE VOLTMETERS FOR MEASURING ALTERNATING POTENTIAL DIFFERENCES.

By W. E. AYRTON and JOHN PERRY.

It is now well understood that a high-resistance electro-dynamometer cannot generally be used for measuring alternating potential differences, in consequence of the self-induction of the dynamometer causing the effective resistance—and therefore the sensibility—of the dynamometer to vary with the speed of alternation. Further, even for the measurement of direct potential differences an ordinary high-resistance voltmeter, employing the magnetic property of the current, is not entirely satisfactory if the forces employed be large (as they generally have to be in dead-beat portable instruments), since, in that case, the current, if kept continuously on, slowly heats the voltmeter, and by increasing its resistance causes its sensibility to diminish. It has, therefore, for some time seemed clear that the heating effect of a current should be used for voltmeters.

Captain Cardew was the first person to utilise this principle in the construction of voltmeters, and, as far as we are aware, is the only person who, up to the present time, has employed the extension of a wire caused by the heating produced by the passage of a current to measure the potential difference at its

terminals. His instruments are well known, and have afforded most valuable aid in the measurement of alternating potential differences; and we, in common with other electrical engineers, owe him a debt of gratitude for providing us with the only commercial instrument that has existed for the measurement of alternating potential differences.

In "Practical Electricity" an attempt has been made to impartially sum up the advantages and disadvantages of all the more important ammeters and voltmeters, and we cannot do better than repeat what was there said about the Cardew voltmeter, especially as we understand that Captain Cardew considers the criticism to be a fair one.

#### "CARDEW VOLTMETER.

*"Advantages.*—First, it has but a small heating error; "second, the self-induction is negligible. It is also dead-beat, "direct-reading, not disturbed by magnets, and fairly portable, "although large.

*"Disadvantages.*—It absorbs a good deal of energy; second, "it cannot be used for measuring a small potential difference, for "we cannot make it of thicker wire, as we should do in the case "of an ordinary voltmeter intended to measure small potential "differences, as this would render it sluggish, since a thick wire "traversed by a current heats and cools slowly on starting and "stopping the current; third, there is considerable vagueness in "the readings near the zero point, and sometimes inaccuracy in "the upper parts of the scale."

Towards the end of 1885 we were engaged on certain experiments on the governing of transformers in series to give a constant potential difference at each house, and, as our experiments were to be conducted with small transformers, we required a voltmeter which would measure an alternating potential difference of three or four volts with accuracy. Such an instrument not being in the market, we were compelled to devise one; and we were led to consider whether the Cardew principle might not be employed in an instrument which should give quite definite readings even near the zero, and produce a large

deflection when an alternating potential difference of a few volts was maintained between its terminals.

Part of the vagueness in the readings of the Cardew instrument we saw was due to the employment of toothed wheels; and, as explained in a paper\* read by us before the Royal Society in 1884, toothed wheels have not recommended themselves to us as a means of magnification in ammeters and voltmeters, in consequence of the friction they are liable to introduce. In fact, after giving toothed gearing an extensive trial in our instruments, originally described to this Society† in 1882, we decided to abandon its use in 1884.

If a voltmeter depending for its action on the expansion of a wire by heating is to be sharp in its action, the wire must be fine; and if it is to measure a small potential difference the wire must be short; hence we were led to consider whether it was not possible to construct a voltmeter of quite a short length of fine wire, and observe its extension with some frictionless magnifying arrangement. This led us to consider the platinum wire telephone described by Mr. Preece in his paper‡ read before the Royal Society in 1880, which, although composed of but a short length of wire, evidenced by the motion of the diaphragm, to the centre of which one end of the wire was attached, every change in the current passing through it. After thinking out various means of attaining this result, we were led to see that one of our magnifying springs, described to this Society in 1882, furnished the means of obtaining a sufficiently sensitive frictionless magnifying arrangement. For experiment shows that if a short piece of fine wire *WW* (Fig. 1) only seven inches long be stretched between two supports *A* and *B* to which it is *rigidly* attached, and if it be pulled slightly out of the straight line by one of our right- and left-handed magnifying springs *M*, *rigidly* attached at its one end to the wire and at the other to a support *S*, the axes of the spring and the wire being at right angles to one another, the

\* "A New Form of Spring for Electric and other Measuring Instruments"

† "Measuring Instruments used in Electric Lighting and Transmission of Power."

‡ "Thermal Effects of Electric Currents."

arrangement is so delicate that the expansion produced by even the approach of a warm hand is evidenced by the rotation of a pointer *P* attached to the centre of the right- and left-handed

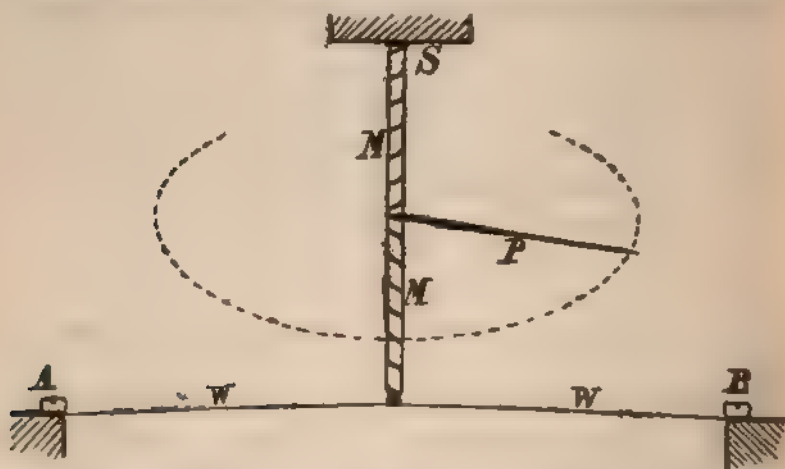


FIG. 1.

magnifying spring. The absence of all gearing and the rigid fastening of both ends of the spring, combined with the fineness of the wire, make the arrangement remarkably dead-beat in its action; and when the wire is enclosed in a short metal tube we have a voltmeter which will measure even the fraction of a volt, whether the potential difference be direct or alternating, since the readings even near the zero are quite definite.

The right- and left-handed spring was employed to avoid any twist being given to the wire; but one of our assistants—Mr. Bourne—who has worked at this instrument with his customary ingenuity and dexterity, soon found out that half the length of the spring could be dispensed with, and an equally efficient but much more compact arrangement could be effected by using a single ordinary magnifying spring *M* (Fig. 2) and introducing a small piece of fine wire *C D* between the one end of the spring and the stretched wire *W W*, or between the other end of the spring and the support *S*.

To get extreme quickness of action on changing the potential difference, it is desirable to employ a thin wire; and as the strength

of such a wire is small, the next step was to combine a number of wires mechanically in parallel, and electrically in series or parallel, the wires being attached at their middles to a stirrup which is

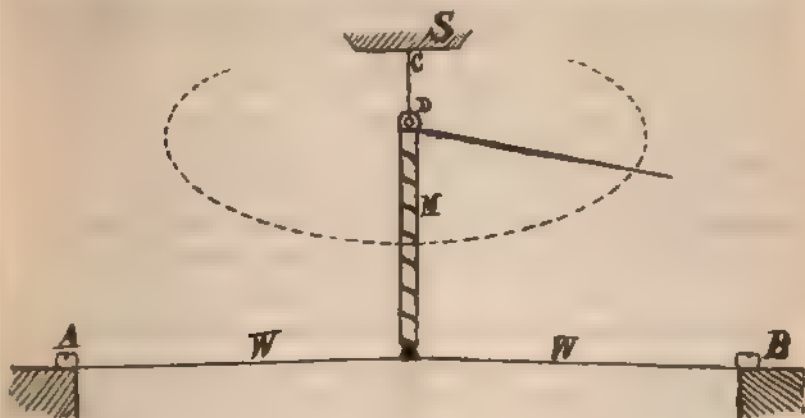


FIG. 2.

carried by the magnifying spring; and voltmeters containing as many as twelve short wires, each about seven inches long, were made in the early part of the summer of 1886.

These instruments showed, however, the sluggishness which is familiar to the users of the Cardew voltmeter, and we had to set ourselves to investigate it. Stationary metallic screens were inserted between the wires to assist the cooling, and various other means tried, without success, when we set Mr. Bourne to investigate the general question of sluggishness. A variety of experiments were made by him on the rate of variation of the permanent state of a wire with the variation of potential difference at its terminals, as affected by varying the proximity of the wires, their length, &c. This occupied him several weeks, and the experiments he made were too numerous to be recorded here. With our many-wire voltmeter the sluggishness was certainly not due to mechanical friction, as our magnifying spring is practically frictionless; and we gradually saw that the peculiar creeping back in the deflection of a Cardew voltmeter when a perfectly constant potential difference was first set up between the terminals was due to the fact that the draught of air created in the tube by the heated wires takes an appreciable time to be produced, so that the wires

are hotter directly the potential difference is applied than they become when the draught begins to flow. A number of more or less successful devices were tried for overcoming this defect—for instance, even the employment of a draught independently maintained—when we found in the summer of last year that the simple device of placing the tube horizontal instead of vertical, and using very fine wire—not much exceeding one-thousandth of an inch in thickness—overcame all the difficulty. The importance of placing the tube horizontal was also subsequently seen by Mr. G. S. Ram this year, and consequently the scales of the Cardew voltmeters are now marked so that the zero is at the top when the tube is placed horizontal.

This home-made instrument with two wires lying on the table was then completed in August of last year, and was found to be remarkably sharp and decisive in its action—to give a deflection of  $300^\circ$  with 14.22 volts maintained at its terminals. Even a fraction of a volt can be measured with it, as the readings are quite definite even near the zero.

The finest wire employed by Captain Cardew in his commercial instrument has a diameter of 0.0025 of an inch; but, in consequence of our magnifying gearing being frictionless and comparatively massless, we are able to use much finer wire—not more than 0.0014 of an inch in diameter—without any fear of injury, even if the instrument receive a sharp knock. This greater fineness of the wire gives us three times as much resistance per foot, and therefore enables the instrument to be very much smaller; also, as the ratio of surface to sectional area is far greater, the heating and cooling is far more rapid—that is, the instrument is much more dead-beat.

Figs. 3 and 4 show two sections at right angles to one another through the centre of this other two-wire voltmeter which is lying on the table. It has been constructed on the lines of the original one, but in a more workmanlike manner. It is direct-reading, and measures from 0 to 10 volts, the deflection for that potential difference being some  $250^\circ$ . The same letters are employed to designate the various parts of the instrument as were used in Figs. 1 and 2, with the addition of *T*, the metal tube surrounding



the wires; and the stirrup connecting the end of the magnifying spring with the wires is also shown.

The employment of several short parallel wires soon suggested

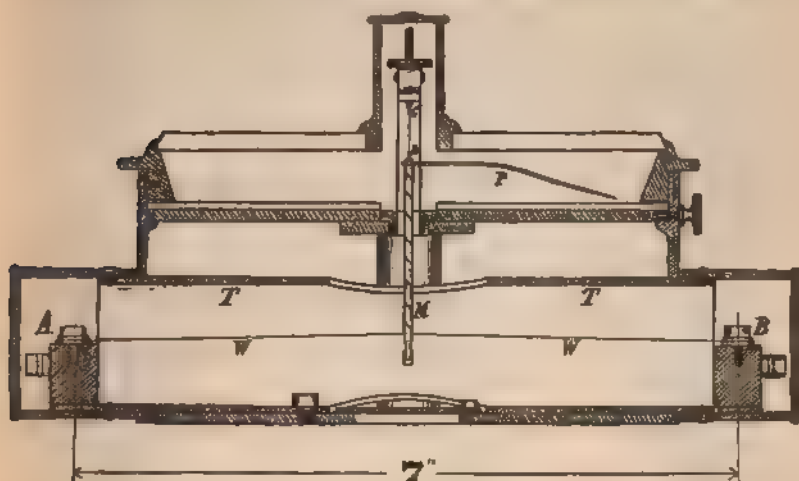


FIG. 3.

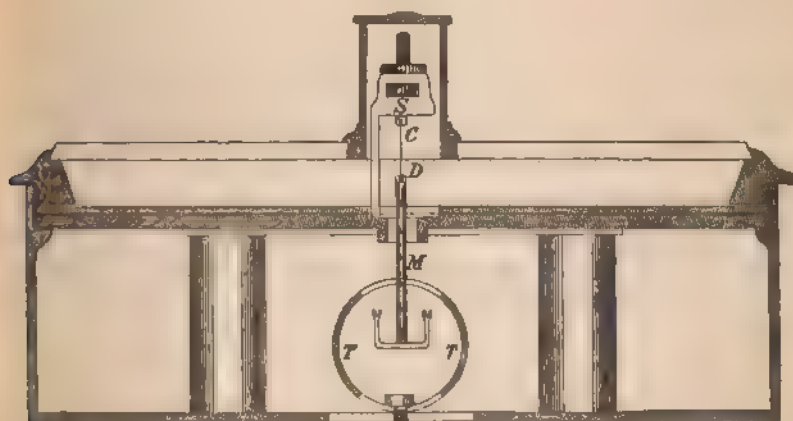


FIG. 4.

the addition of the commutator described in our paper read before this Society in 1881, for varying the sensibility of a voltmeter. But whereas with our commutator ammeters and voltmeters, in which the magnetic action of a current in deflecting a magnetised needle was employed, the current had to flow through every wire in the same direction, whether the wires were



joined in series or in parallel, with this new form of voltmeter in which the heating property is employed it is unimportant which way the current passes through a wire. Hence the pins in the barrel of our old commutator which gave trouble may be entirely dispensed with, and the commutator assumes a very simple form. In fact, if  $AB$ ,  $BC$ ,  $CD$ ,  $DE$ ,  $FG$  (Fig. 5), represent wires

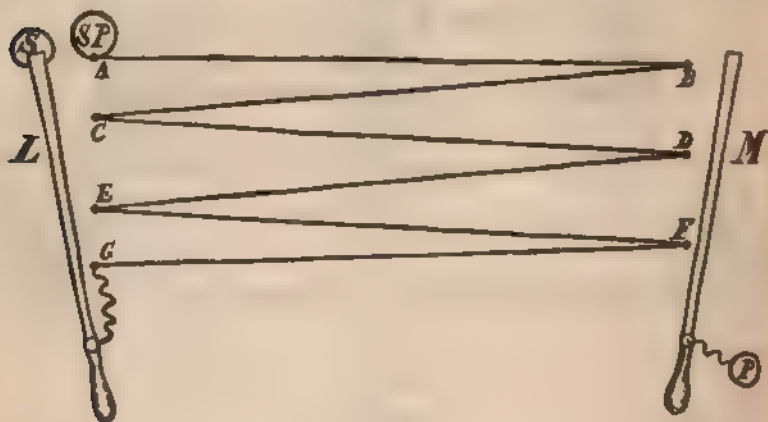


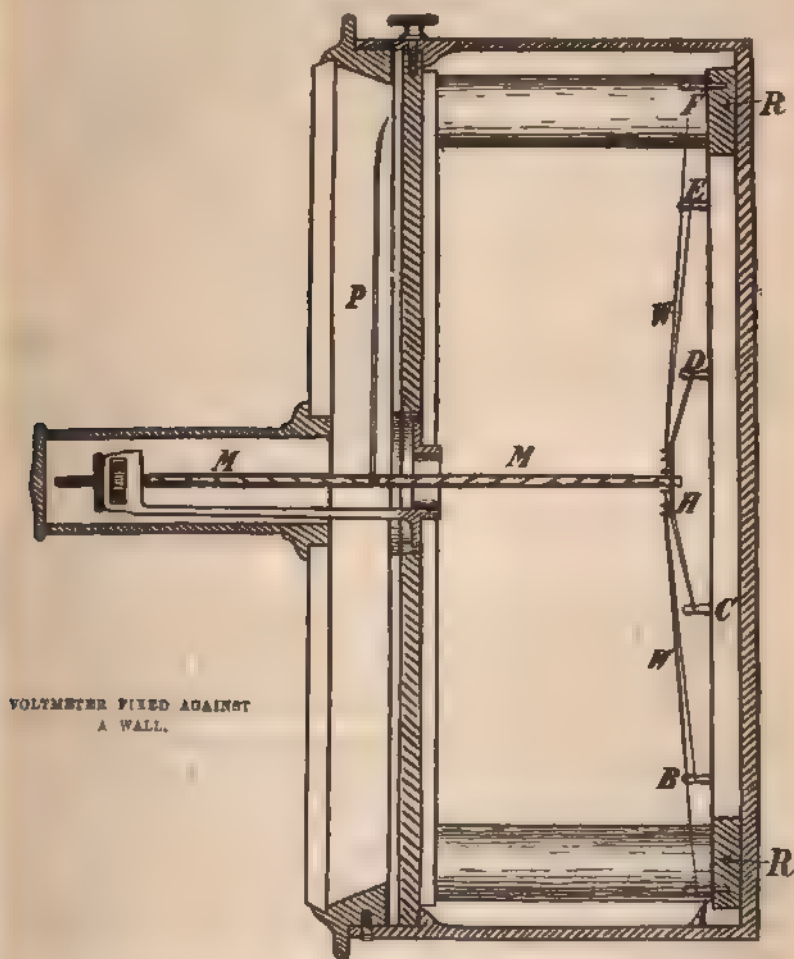
FIG. 5.

joined in series in the new form of voltmeter, the series connections need not be disturbed when a parallel arrangement is required, since all that need be done to join them in parallel is to connect the points  $A$ ,  $C$ ,  $E$ ,  $G$ , by means of the bar  $L$ , and  $B$ ,  $D$ ,  $F$ , by means of the bar  $M$ .  $S$  and  $SP$  are the terminals for series, and  $P$  and  $SP$  for parallel, so that if the commutator be accidentally turned from series to parallel the circuit is broken and the wires are not fused. This instrument lying on the table is a four-wire voltmeter, and the sensibility is such that 15.2 volts gives a deflection of  $39^\circ$  when the commutator is turned to series, and  $295^\circ$  when it is turned to parallel.

It is important to notice that the employment of this commutating device to vary the sensibility enables all the wire in the instrument to be always operative in deflecting the pointer; whereas if the ordinary device of adding an outside resistance coil be employed to enable the voltmeter to measure a larger number of volts, the energy spent in heating this outside

resistance coil is entirely wasted as far as deflecting the pointer is concerned. In fact, the greater the outside resistance, the more inefficient becomes the voltmeter.

For the object of still more completely carrying out the principle of employing a number of short wires we have also made voltmeters in the following way:—The wires are arranged like the spokes of a small bicycle wheel *W W* (Fig. 6); that is, we have a



VOLTMETER FIXED AGAINST  
A WALL.

FIG. 6.

circular rim of a wheel *R*, which may be of the same metal of

which the wires are made, to eliminate errors due to change of temperature, fitted with non-conducting studs *A*, *B*, *C*, *D*, &c., to which the wires are attached. There is also a small non-conducting central piece *H*, corresponding with the hub of a wheel, to which the wires are also attached; and as the wires come from rim to middle backwards and forwards several times, the arrangement appears like a bicycle wheel with many spokes, as seen in Fig. 7.

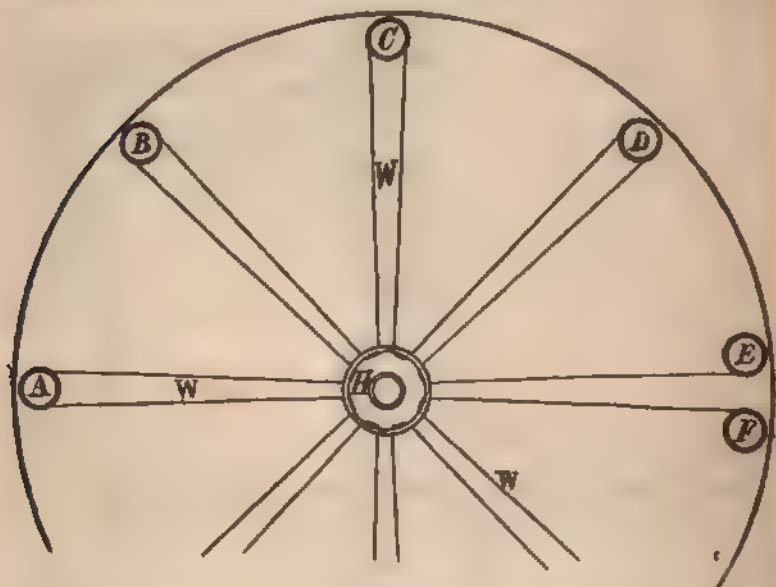


FIG. 7.

One end of a right- and left-handed magnifying spring *M* (Fig. 6) under tension is rigidly attached to the hub *H*, and its other end to the support *S*. Hence, as the wires expand on heating, the spring draws the hub *H* more out of the plane of the rim *R*, and the rotation of the pointer *P* measures the current. This instrument on the table is made exactly in this way. Using two of its three terminals, the range is from 0 to 50 volts; or using one of the previous ones and the third, it is from 0 to 100 volts. In both cases, however, the whole of the wire is operative in deflecting the pointer.

Fig. 8 shows a voltmeter made with two bicycle wheels. The letters attached to one of them refer to the same parts as the

letters used in Fig. 6, while similar accented letters are used for the similar parts of the second bicycle wheel. As before, one end of the right- and left-handed magnifying spring is attached

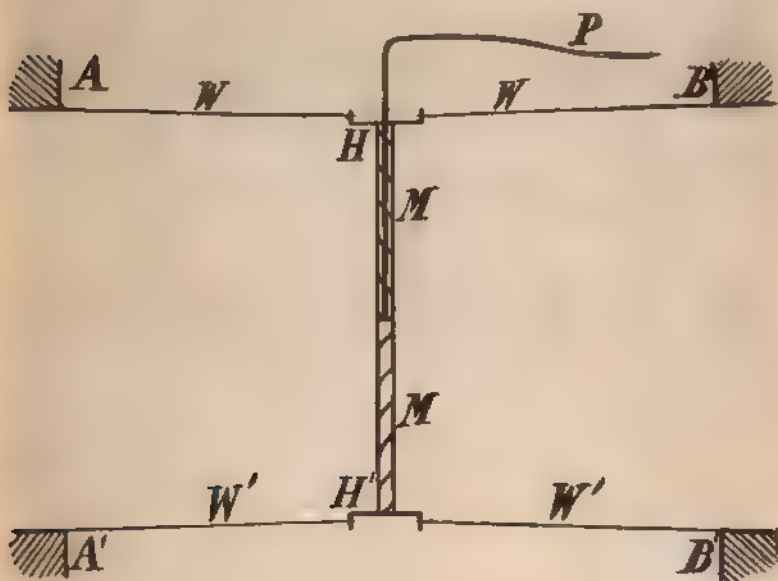


FIG. 8.

to the hub of one of the wheels, but instead of the other end being rigidly attached to the case of the instrument it is attached to the hub of the other wheel. Hence the rotation of the pointer is produced by the expansion of all the wires on both wheels. Fig. 9 shows the actual arrangement of the double bicycle-wheel voltmeter on the table, where an ordinary (not a right- and left-handed) magnifying spring *M* is employed, with a flexible connection of fine wire carried by the support *S*, which itself is carried by the hub *H* of one of the wheels. The wires are electrically divided into four sets. When the commutator is turned to series, 80 volts produce a deflection of  $300^\circ$ ; whereas 20 volts produce a deflection of  $300^\circ$  when the commutator is turned to parallel.

In Fig. 6 of the single bicycle-wheel voltmeter the instrument is shown with the dial vertical. But any one of the

forms described in this paper may also be used with the dial either horizontal or vertical.

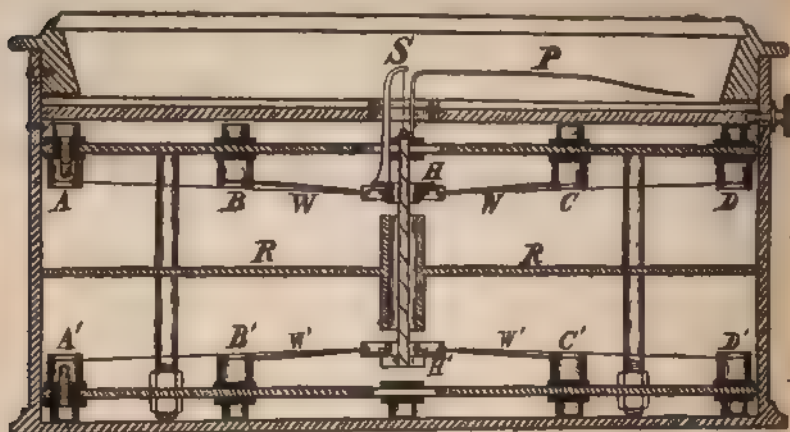


FIG. 9.

The action of these instruments is quite easy to understand; but the mathematical calculation of the way in which the deflection of the pointer for a given potential difference is varied by varying the dimensions of the magnifying spring, the initial sag given to the wire, and the dimensions of the wire, is not quite so easy to work out. We have, however, succeeded in reducing the solution to a comparatively simple form, and the following comprises the consideration of—

- A. The general formulæ and the law of graduation, page 550.
- B. Waste of power in the voltmeter, page 557.
- C. The range in volts of the voltmeter, page 559.
- D. The greatest angular deflection of the pointer, page 559.
- E. Uniformity of the scale divisions, page 562.
- F. Correction for temperature, page 562.

#### A.—GENERAL FORMULÆ AND LAW OF GRADUATION.

Let  $A O B$  (Fig. 10) be one of our fine wires or two opposite

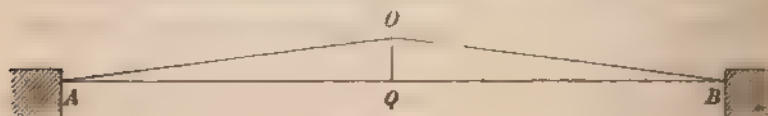


FIG. 10.

spokes of our bicycle-wheel form of voltmeter, the size of the hub being neglected. Let

$$A O = O B = \frac{1}{2} l,$$

$$O Q = y.$$

Let the diameter of the wire be  $d$ , and the specific resistance of its material be  $\rho$  when its temperature is  $\theta$  degrees above the temperature of the surrounding metal case: then, if the case does not alter greatly in temperature,

$$\rho = \rho_0 (1 + k \theta) \dots \dots \dots (1)$$

where  $k$  is the coefficient of increase of resistance per degree centigrade, and  $\rho_0$  is the specific resistance of the material when  $\theta$  is nought.

When	$\theta = 0,$
let	$l = l_0,$
and	$y = y_0;$
then	$l = l_0 (1 + \alpha \theta),$

where  $\alpha$  is the coefficient of increase of length per degree centigrade.

$$\begin{aligned} y^2 + A Q^2 &= \frac{1}{2} l^2, \\ y_0^2 + A Q^2 &= \frac{1}{2} l_0^2; \\ \therefore y^2 - y_0^2 &= \frac{1}{2} l_0^2 \alpha \theta, \text{ approximately} \dots (2) \end{aligned}$$

since  $\alpha$  is small.

In that very useful treatise of Dr. Everett's, "Units and Physical Constants," there are given the results of Mr. McFarlane's and of Professor Tait's experiments on the rate of loss of heat in air at ordinary pressure from copper for various differences of temperature between the copper and the surrounding space, but no indication is given as to the form of the radiating body, nor of its size, and hence people have used Mr. McFarlane's results as if they applied to bodies of any shape and size, and so have been led in some cases to erroneous conclusions. As a matter of fact, Mr. McFarlane's values for the rate of loss of heat by radiation and convection given in Dr. Everett's "Units and Physical Constants" *apply only to a copper ball of 2 centimètres radius*, since this, we find, was the cooling body employed by Mr. McFarlane in the

experiments in question, as mentioned in Mr. McFarlane's paper communicated by Sir W. Thomson to the Royal Society in 1870.

But in a very valuable book published two years previously to that—viz., in 1868—by Mr. Box, and called a "Practical Treatise on Heat," there is a table given on page 151 (which has been much used by engineers) showing the loss of heat from contact with air with horizontal cylinders and spheres. It was deduced, we rather think, from M. Peclet's experiments, and it shows clearly that the loss of heat per unit area increases as the diameter of the cylinder or sphere diminishes. Mr. Box used the Fahrenheit scale for temperature, a square foot as his unit of area, a pound for his unit of mass, and an hour for his unit of time; but, reducing his formulæ to the C.G.S. system, we find that the loss of heat (gramme, C.<sup>o</sup>) per second, per square centimètre of surface, per 1<sup>o</sup> C. excess, is

$$0\cdot00005710 + \frac{0\cdot0001057}{a} \text{ for a long horizontal cylinder,}$$

$$\text{and } 0\cdot00004928 + \frac{0\cdot0003609}{a} \text{ for a sphere,}$$

$a$  being in the former the radius of the cylinder in centimètres, and in the latter the radius of the sphere.

Applying Box's formula for a sphere to the case where  $a$  is 2 centimètres, the radius of Mr. McFarlane's ball, we obtain for the loss of heat (gramme, C.<sup>o</sup>) per second, per square centimètre of surface, per 1<sup>o</sup> C. excess, the number 0·0002297—a number which is between Mr. McFarlane's value for polished copper (0·000178) and for blackened copper (0·000252). There is, therefore, every reason for believing that Box's formula is at any rate approximately correct.

Let us take as a first approximation that the rate of loss of heat is directly proportional to the difference of temperature. This we know is not strictly true, and the error arising from the assumption will be further considered under section B—"Waste of Power in the Voltmeter." Then, if  $\epsilon$   $\theta$  be the heat-power in watts given out per square centimètre of surface of the wire when the excess of temperature is  $\theta^{\circ}$  C.,



$$\epsilon = \frac{0.00005710 + \frac{0.0001057}{\alpha}}{0.239}.$$

Now the resistance of the wire is  $\frac{4 l_o \rho}{\pi d^2}$ ; so that, if  $V$  volts be the potential difference at its terminals,  $\frac{V^2 \pi d^2}{4 l_o \rho}$  watts are developed in the wire, and  $l_o \pi d \epsilon \theta$  = heat-power emitted in watts; so that

$$l_o \pi d \epsilon \theta = \frac{V^2 \pi d^2}{4 l_o \rho} \quad \dots \quad \dots \quad \dots \quad (3)$$

$$\text{or} \quad \theta = \frac{V^2 d}{4 l_o^2 \epsilon \rho} \quad \dots \quad \dots \quad \dots \quad (4)$$

If the thickness of the wire be very small, and if Box's formula continue to hold for very thin cylinders,

$$\epsilon = \frac{0.0001057}{0.239 \alpha}, \text{ approximately;}$$

$$= \frac{0.0008848}{d}, \quad "$$

$$\text{hence} \quad \theta = \frac{V^2 d^2}{0.003539 l_o^2 \rho}, \quad "$$

so that to raise a fine wire of a given length, made of a given material, to a given temperature above that of the case of the instrument requires that the potential difference maintained at the ends of the wire shall be inversely proportional to the diameter of the wire, approximately.

If  $C$  be the current in ampères flowing through the wire,

$$C = \pi d^{\frac{3}{2}} \sqrt{\frac{\epsilon \theta}{4 \rho}}.$$

Substituting the value just obtained for  $\epsilon$  for very fine wires, we have

$$C = 0.01487 \pi d \sqrt{\frac{\theta}{\rho}}, \text{ approximately;}$$

or the current required to maintain a fine wire of a given material at a definite excess of temperature is approximately directly proportional simply to the thickness of the wire.

This result was obtained experimentally by Professor Forbes

in 1884,\* but he appeared to regard it as a result that would not have been expected from experiments previously made on much thicker wires. Mr. Preece also published results,† in the same year, of experiments made on fine wires, and he also speaks of the results of experiments "contradicting" the law which he obtains theoretically. Whereas it appears to us that the current having to vary directly as the diameter, and not as the diameter raised to the power three halves, in order that *very fine* wires may be maintained at the same temperature, is in entire conformity with the true law of the loss of heat by radiation and convection, which was published certainly as early as twenty years ago by Mr. Box.

Equation (5) becomes

$$\theta = \frac{V^2 d}{4 l_o^2 \epsilon \rho_o (1 + k \theta)},$$

or

$$\theta = \frac{V^2 d}{4 l_o^2 \epsilon \rho_o \left(1 + k \frac{V^2 d}{4 l_o^2 \epsilon \rho_o}\right)}, \text{ approximately } (5)$$

One law of the magnifying spring is that

$$\phi = \frac{s}{r} (y - y_o) \quad \dots \quad \dots \quad \dots \quad (6)$$

where  $\phi$  is the rotation of the pointer in radians,  $s$  is a number depending upon the material of which the spring is made, and has a value about 1 radian for phosphor bronze, and  $r$  is the radius of the spring.‡

Substituting for  $y$  from (6) in (2), we have, as

$$y = \frac{r \phi}{s} + y_o,$$

$$\frac{r^2}{s^2} \phi^2 + 2 \frac{r}{s} y_o \phi = \frac{1}{4} l_o^2 \alpha \frac{V^2 d}{4 l_o^2 \epsilon \rho_o \left(1 + k \frac{V^2 d}{4 l_o^2 \epsilon \rho_o}\right)},$$

or

$$\phi^2 + 2 \frac{s}{r} y_o \phi = \frac{s^2 d \alpha}{r^2 8 \epsilon \rho_o} \frac{V^2}{1 + \frac{k d}{4 l_o^2 \epsilon \rho_o} V^2} \quad \dots \quad \dots \quad (7)$$

\* *Jour. Soc. Tel.-Engrs.*, vol. xiii., 1884, page 243.

† *Proc. Roy. Soc.*, No. 231, page 468.

‡ "A New Form of Spring for Electric and other Measuring Instruments," *Proc. Roy. Soc.*, No. 230, 1884, page 306.

or, if  $A = \frac{s^2}{r^2} \frac{d a}{8 \epsilon \rho_0}$ , and  $B = \frac{k d}{4 l_0^2 \epsilon \rho_0}$ ,

and if  $\frac{s}{r} y_0$  be called  $\phi_0$ , then

$$\phi^2 + 2 \phi_0 \phi = \frac{A V^2}{1 + B V^2} \quad \dots \quad (8)$$

and hence

$$\phi = \phi_0 \left\{ -1 + \sqrt{1 + \frac{A V^2}{\phi_0^2 (1 + B V^2)}} \right\} \quad \dots \quad (9)$$

There are discrepancies in this result, due to—

- 1st. Our assuming (1). But it will be found that the error due to this assumption is not large for the usual temperatures of the instrument when platinum-silver wire is employed. See F—"Correction for Temperature."
- 2nd. Our assuming  $\epsilon$  to be independent of  $\theta$ . If  $\epsilon$  is not constant, the law (9) alters in shape. As we never use (9) for the purpose of graduating, but merely to give us some approximate knowledge of a general law, this will not affect in any way the accuracy of the instrument.
- 3rd. We have not taken into account the fact that time is needed to attain the state of equilibrium indicated by (4), so that  $\theta$  might be expected to rise higher on first starting the current than its true steady value; but we find experimentally that this time is quite inappreciable with a *very fine wire placed horizontally*.
- 4th. Our having neglected small changes in  $y$  due to the tensile strain in the wire. It can be proved that these are unimportant, except when  $y_0$  is smaller than we are ever able to have it in our instruments.
- 5th. Our having dealt with certain small quantities as if they were indefinitely small.

$B$  is found to be small in our instruments, and when  $y_0$  is very small it is obvious that the deflection  $\phi$  of the pointer is very nearly proportional to  $V$ , the potential difference. When  $y_0$  is not small, the readings increase at first in proportion to the square of the volts, and then become more nearly proportional to the volts. In fact, the divisions for volts of the scale of the instrument get rapidly greater for small readings; then, as the

readings become higher, the divisions become equal to one another; and if the range is high enough they become somewhat smaller

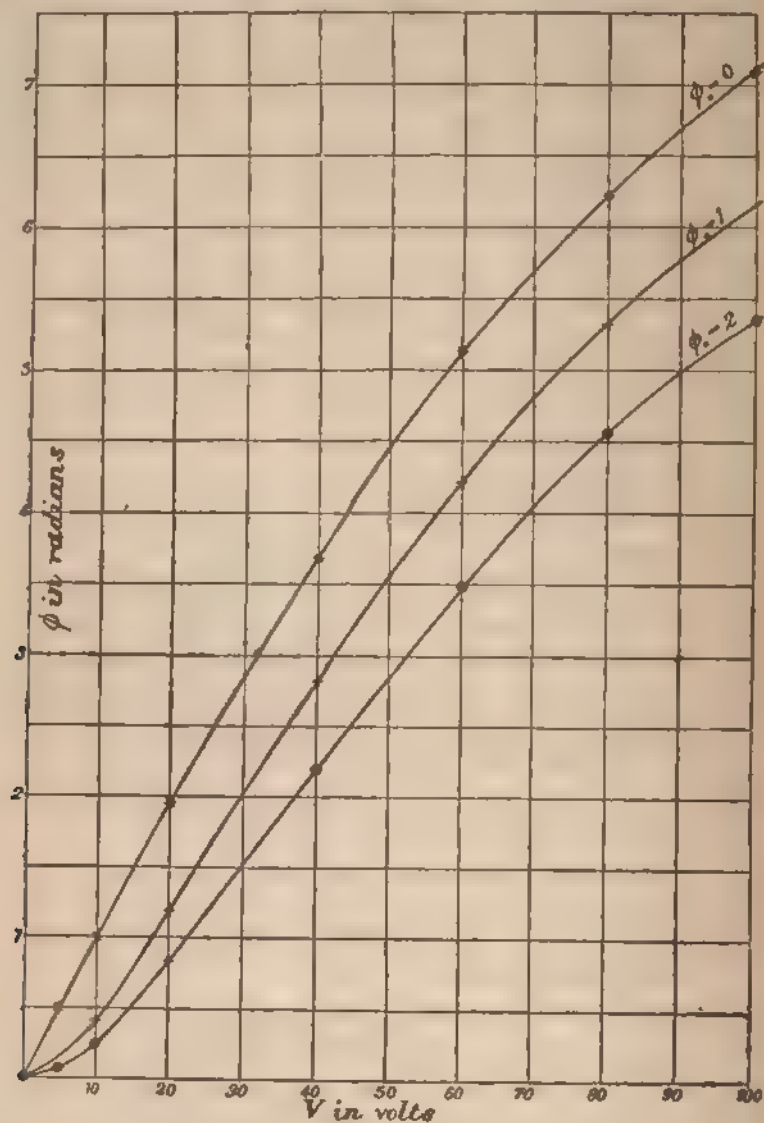


FIG. 11.

again, due to the change of resistance with temperature making its effects felt.

For the sake of illustration we have drawn the three curves shown in Fig. 11, taking

$$A = \cdot 01$$

and  $B = \cdot 0001$ ,

and the values 0, 1, and 2 for  $\phi_0$ .

If such a spring is used that  $\frac{s}{r} = 1$ , then  $\phi_0$  is  $y_0$ .

#### B.—WASTE OF POWER IN THE VOLTMETER.

From the equations previously given it follows that the waste of power in watts spent in keeping a long fine wire  $l_0$  centimètres in length  $\theta^\circ$  C. above the temperature of the surrounding space equals

$$0\cdot0008848 \pi l_0 \theta,$$

a value which depends only on the length of the wire and on the temperature to which it is raised above the temperature of the case, but not at all on the diameter of the wire. This result is confirmed by the experiments recently made by Mr. Evershed, who has found that about 36 watts are required to keep a wire 3,600 mms. long at a temperature of about  $180^\circ$  C. above the temperature of the case, whether the wire be 0.038 mm. or 0.0635 mm. or 0.0889 mm. (i.e., 1.5 or 2.5 or 3.5 mills.) in diameter. Substituting 360 centimètres for  $l_0$  in the above expression, and  $180^\circ$  C. for  $\theta$ , the value becomes 180.2 watts. This is more than four times the value obtained by Mr. Evershed, and the reason may be partly due to the fact that (as already noticed in section A) we have hitherto regarded the rate of loss of heat as being directly proportional to the difference of temperature. On turning to Mr. McFarlane's paper we find his result may be expressed as follows:—

Loss of heat per second per square centimètre of a bright copper ball 2 centimètres in radius for a difference of temperature  $\theta^\circ$  C.

$$= \text{Loss for } 1^\circ \text{ C.} \times \theta \left( \frac{0\cdot000168}{0\cdot000170} + \frac{1\cdot98}{0\cdot000170} \times 10^{-6} \theta - \frac{1\cdot7}{0\cdot000170} \times 10^{-8} \theta^2 \right).$$

Instead, therefore, of writing as above the expression for the rate of loss of heat in watts required to maintain a long wire

$l_0$  centimètres in length at a temperature  $\theta^\circ$  C. above that of the surrounding space, it will probably be more correct to write it as something like

$$0.0008848 \pi l_0 \theta \left( \frac{0.000168}{0.000170} + \frac{1.98}{0.000170} \times 10^{-4} \theta - \frac{1.7}{0.000170} \times 10^{-4} \theta^2 \right).$$

If now we substitute 360 centimètres for  $l_0$  in this expression, it becomes

$$\theta (0.9883 + 0.01165 \theta - 10^{-4} \theta^2) \text{ watts.}$$

Of course we cannot expect this expression to give absolutely correct results for any values of  $\theta$ , seeing that Mr. McFarlane's experiments did not extend beyond  $\theta$  equal  $60^\circ$  C. The expression is interesting, however, as showing that, in consequence of the negative term,  $\epsilon$  must be taken as having a smaller value when  $\theta$  is high than when  $\theta$  is low. For  $\theta$  equal to  $180^\circ$  C., which was about the excess temperature of the wire in Mr. Evershed's experiments, the formula as it stands cannot be applied, as it leads to a negative answer; but for  $\theta$  equal to  $164.5^\circ$  C. it gives, as it happens, almost exactly 36 watts, the power Mr. Evershed found was expended in any one of the three wires he tried.

Mr. Evershed informs us that he estimates the temperature to which the wire is raised by measuring its expansion; we, on the other hand, have found it more convenient, with our form of voltmeter, to estimate the rise of temperature of the wire by measuring its increase of resistance. Employing this latter method, we find that, roughly, 3.5 watts per foot are necessary to maintain a very fine platinum-silver wire at a temperature of  $200^\circ$  C. above that of the case. This result is rather higher than that obtained by Mr. Evershed, and corresponds with a value of  $\epsilon$  equal to about  $\frac{0.0002}{d}$ .

In the absence of the results of experiments made for many values of  $\theta$  it is impossible to say what is the exact function of  $\theta$  that ought to be employed in the expression for the watts; but from what we have given there is every reason to think that the expression for the watts required to be spent in maintaining a long fine wire at any temperature  $\theta$  above that of the surrounding space is a function of the length of the wire and the excess

temperature only, and is nearly, if not entirely, independent of the diameter of the wire.

### C.—THE RANGE IN VOLTS.

If  $\theta_1$  be the highest temperature of the wire allowable; if  $V_1$  volts be the range of the instrument without using external resistance coils, then, from (4),

$$V_1 = 2 l_o \sqrt{\epsilon \frac{\rho \theta_1}{d}} \quad \dots \quad \dots \quad (10)$$

so that this range is proportional to  $l_o$ —that is, to the length of wire through which the current passes in going from one terminal to another of the instrument. Hence, if there are 10 wires, each of length  $\lambda_o$ , we can make  $l_o = 10 \lambda_o$  by placing the wires in series, or  $l_o = \lambda_o$  by placing them in parallel by means of a commutator.

Since, for small wires,  $\epsilon$  is inversely as  $d$ , the range in volts increases directly as the length of the stretched wire and inversely as its diameter. As, however, increasing the length increases the waste of power in the instrument, it is rather by diminishing the diameter of the wire that the range in volts ought to be increased.

If a small range in volts be desired, it is better, in order to avoid waste of power, to use one thick wire than several thin wires in parallel; and this rule ought to be followed as long as the thick wire is sufficiently quick in its action.

In order, however, to obtain a great range, it may in many cases be desirable to employ a many-wire voltmeter with a commutator, in spite of the fact that this arrangement does not give us the instrument that is most economical in the power wasted when a few volts are being measured.

### D.—THE GREATEST ANGULAR DEFLECTION OF THE POINTER.

With any given spring, it is obvious that we ought to tighten the wire and adjust the initial tension in the spring until the initial pull in the wire is  $q$ —the greatest pull to which the wire ought to be subjected—and also until we find that at the highest temperature the spring is just able to keep the wire taut. In our instruments where the wires lie nearly all parallel to one another



we have these two adjustments, and also in those with the bicycle-wheel arrangement of wires, in which the outer ends of the wires may be attached to adjustable flexible strips. For the sake of easy calculation we may take the final pull in the spring as 0. Let  $L$  be the length of the spring,  $\phi$ , the deflection of the pointer when  $\theta$ , (the highest temperature of the wire) is reached,  $P_0$  the initial pull in the spring. By the triangle of forces

$$P_0 = \frac{4 y_0 q}{l_0} \quad \dots \quad \dots \quad \dots \quad (11)$$

(If there are  $n$  wires caught by the end of the spring, say  $n$  complete diametral wires in the bicycle-wheel form, then instead of  $q$  we must use  $n q$ .)

The law of the spring is such that

$$y_1 - y_0 = r P_0 L \frac{a}{t^3} \dots \quad \dots \quad \dots \quad (12)^*$$

where  $r$  is the radius of the coils of the spring,  $L$  its axial length  $a$  a constant depending on the material of the spring,  $t$  the thickness of the strip of which the spring is made; and another law of the spring is

$$\phi_1 = P_0 L \frac{b}{t^3} \quad \dots \quad \dots \quad \dots \quad (13)$$

where  $b$  is a constant like  $a$ . In fact,  $\frac{b}{a}$  is the constant  $s$  used in (6).

Substituting for  $P_0$  from (11) in (12),

$$y_1 - y_0 = r L \frac{a}{t^3} \frac{4 q}{l_0} y_0 = S \frac{r}{t^3} y_0, \text{ say} \quad \dots \quad (14)$$

equation (2) is  $y_1^2 - y_0^2 = \frac{1}{2} l_0^2 a \theta_1 = R$ , say.

Dividing by (14), we have

$$y_1 + y_0 = \frac{R t^3}{S r y_0};$$

so that

$$y_1 = \frac{R t^3}{S r y_0} - y_0.$$

Substituting in (14), we find

$$\frac{R t^3}{S r y_0} - 2 y_0 = S \frac{r}{t^3} y_0,$$

or

$$y_0 = \sqrt{\frac{R}{S \frac{r}{t^3} \left( S \frac{r}{t^3} + 2 \right)}}.$$

\* "A New Form of Spring for Electric and other Measuring Instruments," *Proc. Roy. Soc.*, No. 230, 1884, p. 311.

Substituting for  $P_0$  from (11) in (13),

$$\phi_1 = \frac{L b}{t_3} \frac{4 q}{l_0} y_0 \dots \dots \dots (15)$$

and using  $y_0$ , just found, in which  $R = 2 l_0^2 \alpha \theta_1$ , and  $S = \frac{4 L \alpha q}{l_0}$ , we find eventually that

$$\phi_1 = \sqrt{\alpha \theta_1 \frac{b^3}{\alpha} \frac{L q l_0^3}{2 L \alpha q r^3 + r t^3 l_0}} \dots \dots (16)$$

where neither of the two terms in the denominator is negligible. The observation in brackets given after (11) tells us that we may use  $nq$  instead of  $q$  in the formula if there are  $n$  lengths of wire or  $n$  complete diametral wires in the bicycle-wheel form of instrument. An examination of (16) will show how the range  $\phi_1$  may be made large.

The following are the deductions that may be drawn from (16):—

1st. If  $f$  be the working stress suitable for the material of the wire,

$$q = f \frac{\pi}{4} d^2,$$

or

$$n q = n f \frac{\pi}{4} d^2;$$

so that, if we desire a large maximum deflection without reference to the range of volts, we ought to use wires as thick as possible, or else use many spokes in the bicycle-wheel form. The limit to the thickness that may be given to the wires arises from the fact that the heat equilibrium will only be established slowly with thick wires, so that the dead-beat character of the instrument suffers if the wires be too thick.

2nd. The greater  $l_0$  is, the greater is the maximum deflection. Hence the greater the diameter of the bicycle wheel and the more numerous the spokes, the greater the maximum deflection. Also, the greater the diameter of the bicycle wheel and the greater the number of spokes, still more noticeable is the range in volts.

3rd. Diminishing the thickness ( $t$ ) of the strip of metal of which the spring is made has a *very great* effect in increasing the maximum deflection.

- 4th. Increasing the length ( $L$ ) of the spring increases the maximum deflection just in the same way as increasing  $q$  or  $nq$ .  
 5th. Diminishing  $r$ , the radius of the coils of the spring, produces a great increase in the maximum deflection.

#### E.—UNIFORMITY OF THE SCALE DIVISIONS.

It is rather important to notice that from (9) it follows that the smaller is  $\frac{s}{r} y_0$  the more uniform become the divisions of the scale.

Now, from (15),  $y_0 = \frac{t^3 l_0}{4 L b q} \phi_1$ ; so that  $\frac{s}{r} y_0$  being small means, since  $s = \frac{b}{a}$ , that  $\frac{1}{a r} \frac{t^3 l_0}{4 L q} \phi_1$  is small, or that  $\frac{t^3 l_0 \phi_1}{4 L q a r}$  is small. But as a large maximum value of the deflection is very important, and as this, as we have seen, requires that  $t$  and  $r$  should be small, and that  $l_0$  and  $q$  should be large, the only way in which we can simultaneously satisfy both sets of conditions is to make  $nq$  large—that is, by using thick wires, many wires (as many wires as possible), and very thin strips for our magnifying spring. Using thick wires, however, diminishes the range of volts that can be measured.

Finally, it is to be observed that the condition of most importance in obtaining

- (1) A large maximum deflection,
- (2) Uniformity of the divisions of the scale,
- (3) A large range of volts that can be measured,

is to make the strip of which the magnifying spring is made as thin as possible.

#### F.—CORRECTION FOR TEMPERATURE.

In what precedes we have disregarded the effect produced by the variation of the temperature of the instrument as a whole, caused partly by variations of atmospheric temperature, and partly by the warming up of the whole instrument by a prolonged maintenance of the potential difference at its terminals. We will now investigate the magnitude of the error so caused.

Let  $\alpha$  be the coefficient of expansion of the stretched wire;  
 $\beta$  be the coefficient of expansion of the framework supporting the ends of the wire;  
 $y_0$  be the dip  $OQ$  (Fig. 10) at the temperature of graduation;  
 $y_t$  " "  $t^\circ$  C. above " " "  
 $y$  " "  $\theta + t^\circ$  C. " " " "

then  $y_0^2 + 4AQ^2 = \frac{1}{4}l_0^2$ ,  
 $y_t^2 + 4AQ^2(1 + \beta t)^2 = \frac{1}{4}l_0^2(1 + \alpha t)^2$ ,  
 $y^2 + 4AQ^2(1 + \beta t)^2 = \frac{1}{4}l_0^2\{1 + \alpha(\theta + t)\}^2$ ;  
 $\therefore y^2 - y_0^2 = \frac{1}{4}l_0^2\alpha\theta + \frac{1}{2}t(l_0^2\alpha - 4AQ^2\beta)$ .

If the initial dip at the temperature of graduation be small,

$$4AQ^2 = l_0^2, \text{ approximately;}$$

$$\therefore y^2 - y_0^2 = \frac{1}{4}l_0^2\{\alpha(\theta + t) - \beta t\}.$$

Let

$$\alpha - \beta = \gamma,$$

where  $\gamma$  can be made positive, nought, or negative by compensation of the framework of the instrument for changes of temperature. Hence

$$y^2 - y_0^2 = \frac{1}{4}l_0^2(\alpha\theta + \gamma t) \quad \dots (17)$$

Also, if  $\rho_0$  be the specific resistance of the material of which the wire is made at the temperature of the framework of the instrument during graduation; and if  $\rho$  be the specific resistance when the framework of the instrument is at a temperature  $t^\circ$  C. above that of the temperature of graduation, and when the wire has a temperature  $\theta^\circ$  C. above that of the framework,

$$\rho = \rho_0\{1 + k(\theta + t)\}.$$

Consequently, following the reasoning previously given, connecting the rate of loss of heat by radiation and convection, and the rate of production of heat by the passage of the current, we have

$$\theta = \frac{V^2 d}{4l_0^2 \epsilon \rho_0 \{1 + k(\theta + t)\}}, \text{ approximately.}$$

Substituting in (17) the value for  $y$  given in (6), and for  $\theta$  the value just obtained, we have

$$\begin{aligned} \phi^2 + 2\frac{B}{\gamma}y_0\phi &= \frac{s^2}{\gamma^2} \frac{1}{2}l_0^2 \left( \alpha \frac{V^2 d}{4l_0^2 \epsilon \rho_0 \{1 + k(\theta + t)\}} + \gamma t \right) \\ &= \frac{s^2 d \alpha V^2}{\gamma^2 8 \epsilon \rho \{1 + k(\theta + t)\}} \left( 1 + \frac{\gamma t}{\alpha \theta} \right) \\ &= \frac{d V^2}{(1 + k\theta) \left( 1 + \frac{k t}{1 + k\theta} \right)} \left( 1 + \frac{\gamma t}{\alpha \theta} \right), \end{aligned}$$

$$\text{or } \phi^2 + 2 \phi_0 \phi = \frac{A V^2}{1+k\theta} \left( 1 - \frac{k t}{1+k\theta} + \frac{\gamma t}{a\theta} \right), \text{approximately (18)}$$

Now formula (8), obtained on the assumption that the framework of the voltmeter always remained at the temperature it was at during the graduation of the instrument, may be written

$$\phi^2 + 2 \phi_0 \phi = \frac{A V^2}{1+k\theta};$$

hence the effect of the whole instrument being heated up to  $t^\circ$  C. is to diminish the right-hand side of the equation by the fraction  $\frac{k t}{1+k\theta}$  of itself, in consequence of the increase of the resistance of the stretched wire; and to increase the right-hand side of the equation by the fraction  $\frac{\gamma t}{a\theta}$  of itself, in consequence of the difference of the coefficients of expansion of the stretched wire and the framework. If  $\gamma$ , the difference between the coefficients of expansion of the stretched wire and of the framework, be selected so that

$$\gamma = \frac{a k \theta}{1+k\theta},$$

then for that particular value of  $\theta$  there will be no temperature error of the instrument. Now the effect of  $\gamma$  not being nought will be to cause a slight motion of the pointer when the whole instrument is warmed up as a whole without any potential difference being maintained at its terminals; or, in other words,  $\gamma$  produces what may be called a zero error. Hence the zero error can be made such that there is no temperature error when the instrument is required to measure a certain definite potential difference or a potential difference of about that value. If, on the other hand, the framework of the instrument be so constructed that  $\gamma$  is nought—that is, so that there is no zero error—then the deflection corresponding with any given number of volts maintained at its terminals will be somewhat diminished as the instrument warms up as a whole. In order that accurate readings may be able to be made of small potential differences giving deflections near the zero it will be desirable to make  $\gamma$  small, but whether it

ought to be made absolutely nought or not can only be determined by practical experience of this new form of voltmeter extending over some time.

Solving (18), we have

$$\phi = -\phi_0 + \sqrt{\phi_0^2 + \frac{A V^2}{1 + k\theta} \left(1 - \frac{k t}{1 + k\theta} + \frac{\gamma t}{a\theta}\right)} \quad (19)$$

and in order to estimate the magnitude of the error arising from the terms involving  $t$  let us consider the values of the constants in the previous expression. In some of our instruments we find that the initial sag  $y_0$  is about 0.5 centimetre, and that  $y - y_0$  is about 0.22 centimetre when  $\phi$  is about  $280^\circ$ —that is, when the needle is deflected to the limit of the scale. Therefore, since

$$\phi_0 = \frac{a}{r} y_0,$$

and 
$$\phi = \frac{a}{r} (y - y_0),$$

it follows that 
$$\phi = \frac{\phi_0}{2}, \text{ approximately,}$$

when the deflection has about its maximum value. Hence, if we write  $X$  for  $\frac{A V^2}{1 + k\theta}$ ,  $X'$  for  $\frac{A V^2}{1 + k\theta} \left(1 - \frac{k t}{1 + k\theta} + \frac{\gamma t}{a\theta}\right)$ , and  $\phi'$  for the value of  $\phi$  when  $X$  becomes  $X'$ , we have

$$\phi = -2\phi + \sqrt{4\phi^2 + X}, \text{ approximately,}$$

and 
$$\phi' = -2\phi' + \sqrt{4\phi'^2 + X'}, \quad ,,$$

therefore the proportional error when the deflection is a maximum

$$\begin{aligned} \frac{\phi - \phi'}{\phi} &= \frac{\sqrt{\frac{X}{5}} - \sqrt{\frac{X'}{5}}}{\sqrt{\frac{X}{5}}}, \quad \text{approximately,} \\ &= 1 - \sqrt{1 - \frac{k t}{1 + k\theta} + \frac{\gamma t}{a\theta}} \quad , \end{aligned}$$

Now  $k$  equals 0.00031 and  $a$  about 0.0000145 for platinum-silver; also  $\theta$  is about  $250^\circ$  C. when the deflection  $\phi$  is about  $280^\circ$ . Let  $t$ , the temperature through which the case of the instrument is raised above the temperature of graduation, be  $50^\circ$  C.: then, if  $\gamma$  be nought,

$$\frac{\phi - \phi'}{\phi} = 0.007, \text{ approximately ;}$$

that is,  $\phi$  is diminished by about 0.7 per cent., in consequence of the increase of resistance of the stretched wire. And this temperature error can be made still less by giving  $\gamma$  a negative value—that is, by making the framework of the instrument have a larger coefficient of expansion than the stretched wire. For instance, if the framework which carries the stretched wire have a coefficient of expansion rather less than that of copper, the temperature error of the voltmeter in the higher parts of the scale will be practically nought.

The accuracy of the instrument depends, among other things, on the constancy of the elasticity of our magnifying springs. To test this we have had tests conducted regularly during the last two years on one of our magnifying spring ammeters. The current was measured absolutely on each occasion by the silver-deposit method, and, as this can be done more easily and accurately if the current be rather small, the ammeter selected for this series of tests was one that gave the maximum deflection (of about  $270^\circ$ ) for  $1\frac{1}{2}$  ampères. Of course the only difference between this ammeter and a magnifying spring ammeter measuring a much larger current is in the gauge of wire used in the winding, and not at all in the spring; therefore any conclusions as to constancy that may be drawn from the results of the tests are equally applicable to any other magnifying spring ammeter. Sixty-four distinct tests have been made by different groups of students in the two years; and to impress on the students the importance of making such time tests of an ammeter, as well as to prevent their imagining that the silver-deposit test ought to give the same values for the currents as are indicated by the graduations of the scale of the ammeter, the students were warned that the ammeter had been intentionally graduated with an error—the error, in fact, being that the ammeter reads from  $1\frac{1}{2}$  to 2 per cent. too low.

Instead of giving the tests in the exact order in which they were taken, each group of tests is, for convenience of comparison, arranged in chronological order.



the tests were made with a Magnifying Spring Ammeter initially graduated from  $1\frac{1}{2}$  to 2 per cent. too low.

DATE.	Temperature in Degrees Centigrade.	Reading on Ammeter.	Ampères by Silver Voltmeter.
October, 1885 ... ..	...	0.588	0.592
July, 1886 ... ..	21°	0.58	0.59
October, 1886 ... ..	21°·8	0.58	0.596
October, " ... ..	17°·5	0.59	0.571
November, " ... ..	15°·8	0.5	0.514
October, 1885 ... ..	...	0.65	0.682
October, " ... ..	...	0.65	0.606
March, 1886 ... ..	19°	0.600	0.615
" " ... ..	13°·8	0.64	0.657
" " ... ..	14°	0.68	0.692
July, " ... ..	22°·8	0.603	0.683
October, " ... ..	17°·5	0.620	0.635
" " ... ..	17°·5	0.67	0.688
November, 1886 ... ..	13°	0.60	0.615
October, 1887 ... ..	16°·4	0.60	0.618
August, 1885 ... ..	...	0.76	0.776
" " ... ..	...	0.76	0.776
" " ... ..	...	0.757	0.775
November, 1885 ... ..	12°	0.700	0.721
" " ... ..	...	0.721	0.744
December, " ... ..	8°	0.70	0.72
February, 1886 ... ..	14°	0.700	0.727
" " ... ..	16°·1	0.720	0.746
July, 1886 ... ..	20°·5	0.722	0.749
October, 1887 ... ..	18°·4	0.70	0.711
" " ... ..	16°·5	0.70	0.716
October, 1886 ... ..	15°·6	0.84	0.859
February, 1887 ... ..	15°·6	0.80	0.820
October, " ... ..	16°	0.80	0.821
" " ... ..	16°·5	0.80	0.828
" " ... ..	17°·25	0.80	0.813
November, " ... ..	16°·6	0.80	0.814
" " ... ..	16°·6	0.84	0.861
February, 1887 ... ..	13°·5	0.90	0.916
March, 1887 ... ..	12°·2	0.90	0.920
November, 1887 ... ..	18°·8	0.91	0.929
July, 1885 ... ..	...	1.06	1.073
December, 1885 ... ..	...	1.008	1.022
" " ... ..	9°	1.07	1.096
" " ... ..	7°·5	1.12	1.14
" " ... ..	14°	1.12	1.14
October, 1886 ... ..	14°·6	1.147	1.160

as the tests were made by a large number of different students, of very different aptitudes for such work, some of the results

arrived at were obviously wrong, and have been rejected; but, excluding these few exceptional ones, the preceding are a fair sample of the results obtained, and we see that it is impossible to conclude from them whether the ammeter has become more or less sensitive during the two years. In other words, its sensibility for practical purposes has remained quite constant. The temperature was carefully recorded after the early part of 1886, since it was thought that perhaps some temperature error might become visible; but this has not turned out to be the case, so that *the temperature may be apparently neglected in using a magnifying spring ammeter*. With a magnifying spring voltmeter wound with copper wire there will, of course, still remain the error due to a variation of the resistance of the coil with temperature; but the important result that has been arrived at is that *the indications of a magnifying spring ammeter or voltmeter, when a given current is passing through it, is not altered by age or by the ordinary changes of temperature of a room*.

To further test this latter point a magnifying spring ammeter, adjusted so that the needle stood exactly at zero when the temperature was about  $15^{\circ}\text{C.}$ , was placed in a bath and heated up to  $40^{\circ}\text{C.}$  for five hours; but no change in the zero could be observed, which would have been the case if the elasticity of the spring had sensibly varied, seeing that the weight of the soft iron cylinder was hanging at the end of the spring all the time. We feel, therefore, quite safe in using the magnifying springs for our new type of voltmeter.

We are now engaged in devising various means for compensating this new form of voltmeter for temperature errors; but before actually introducing these compensations into the instruments one of our students—Mr. Kilgour—has been carefully measuring the exact coefficient of expansion with temperature of fine platinum-silver wire under a tension which is comparable with its breaking tension. The compensation for the short-tube form shown in Figs. 3 and 4 will probably in part consist in the device adopted by Captain Cardew of making the support to which the wires are attached of a compound metal; while in the case of our bicycle-wheel form the rim to which the wires are

attached will probably be made like the balance-wheel of a chronometer; but, of course, the same accuracy of adjustment for temperature that is necessary in a chronometer will be quite unnecessary in the voltmeter, as the error is not a cumulative one as it is in a chronometer.

The following, we venture to think, are the improvements that we have been led to introduce into the Cardew voltmeter by employing the devices that we have described:—

1. A small alternating potential difference can be accurately measured, as the wire in our instrument may be short, and as the readings can be taken down to the zero with certainty.
2. With the same instrument a wide range of volts can be measured, since with many short wires combined with a commutator any number of them can be put in series; that is, any length of wire can be electrically used.
3. Whatever number of volts is being measured, all the wire is directly active in deflecting the pointer, which is not the case with an instrument provided with an external resistance for increasing the range.
4. The instrument is compact and portable.
5. Great dead-beatness of action is secured by the employment of much finer wire than has hitherto been commercially used, and by the magnifying gearing being frictionless and comparatively massless.
6. Non-liability of even such fine wire to break from a blow, in consequence of the frictionless and comparatively massless character of the magnifying gearing to which the wire is attached.
7. Diminution in the power wasted in the instrument by the employment of a short instead of a long wire.

The PRESIDENT: We have heard these two papers read, and I <sup>The President,</sup> shall be glad to hear any remarks upon them.

Mr. J. E. H. GORDON, B.A.: It is difficult to criticise such <sup>Mr. Gordon,</sup> beautiful instruments as Dr. Fleming has devised for us; and if I make any criticism, it must be understood that it is with the

Mr. Gordon feeling of how much we owe to those who will devise us instruments for the practical measurement of volts, and how important it is that every little practical difficulty should be eliminated as fully and as soon as possible. I propose to speak from the point of view of what is required for the engine-room voltmeter. I am not speaking of what is wanted for laboratory purposes or for general testing—these are special requirements; but what we want is a voltmeter that can be placed on circuits, and can be as easily read as an ordinary steam gauge. We want such voltmeters, not as a skilled electrician or skilled workman, or even a fairly intelligent workman, can read, but such as an engine-driver at 30s. a week can be trusted to attend to. If you have a large electric lighting station, with different series of circuits which have to be regulated, you do not want to make it necessary for the man to understand the theory of the instrument, but to give him a dozen or two gauges similar to steam gauges, and tell him to treat them like steam gauges, except that the marks on the electrical gauge are called volts instead of lbs., and he must keep them to, say, 150 volts, or divisions; and without knowing the value of the volt he will understand that on turning the handle to the left his number will go up, or to the right it will go down, without concerning himself whether by turning such handle he puts in resistance, sections of wire or accumulators. Something is wanted that is extremely simple and easily looked at. For this reason one or two criticisms occurred to me at once on Dr. Fleming's paper. The first criticism—which may seem a very trifling one, but yet which I think will not be without practical value—is that in Dr. Fleming's instrument the dial must necessarily be horizontal. You cannot get a man to watch all night and attend to fifteen or twenty horizontal dials, because, if he dozes for a minute, or sits down in a chair, he does not notice them, and has to be continually leaning over a table. But if he has them set in a row opposite him there is very little excuse for his mistaking them, and therefore an instrument which can be placed vertically is much preferable, when managed by an ordinary workman, to a dial which must be placed horizontally. To leave that point for a moment, there is one small practical objection

that I see to Dr. Fleming's instrument; it is, in fact, two objections in one. First of all, the current passes through the spring. I made a note at the time Dr. Fleming stated this, but he afterwards met my objection to some extent by showing that the expansion of the spring partly counterbalanced the different resistances of the coils; but still he will have one-fifteenth ampère going through—I think he said 1,500 ohms, and that with 100 volts gives one-fifteenth part of an ampère. I should imagine that that would give a very considerable current-density in the spring. I do not know what the section of the spring is, but I imagine it must be sufficient to give some hundreds of ampères per square inch, and that has a perceptible warming effect. I do not profess to be an expert in springs, but I should imagine that the warmth, in some degree, must alter the tension of that spring.

There is one other point which I would suggest for Dr. Fleming's consideration. It is that if by any chance negligence the instrument was put away—i.e., the lid shut and the fixing lever screwed down to lift the instrument from its pivot—before the current was taken off, a spark would be got on the point and damage it; and it seems to be not difficult to make a locking arrangement to make it impossible to lift the coils before the current is broken. But no doubt Dr. Fleming has dealt with that small point.

Now, with regard to the Cardew voltmeter, if the President will allow me to bring the two papers together, as I can hardly help it in the general question. It is very delightful to see such a beautiful development of Captain Cardew's principle as Professor Ayrton has shown us; but I think, however much we are pleased with this development, we must not forget what we owe to Captain Cardew in having first given us the principle; and we must remember that that was not a mere theoretical principle, but an instrument in a practical working form. My own experience of the Cardew voltmeter is extensive. For eleven months I had eighteen instruments with the current constantly on them, day and night, never off for a minute; they stood in the engine-room and guided the men in regulating the different

Mr. Gordon, circuits, and they practically worked perfectly, but they required a certain amount of attention. The first point with them was that we had to take them to pieces and remake them the workmanship was not up to the principle of them; but we did that in our own laboratory, and that we will pass over. Then, in graduating them, we always found it necessary to keep the current on at least two or three days—the current making a deflection about equal to the 100 volts or 150 volts that we wanted—so that the wire should get rid of any permanent set or sag it had. We did not trouble about zero—we did not want that; we only wanted to know, say in a 150-volt circuit, when it was 145, 150, or 155. Even in that part of the scale they were never quite constant, and had to be reset from time to time. We could not interfere with the instrument as regards altering the tension of the wire, and I took the liberty of taking off the dials: I pivoted them, and arranged so that we could turn the dial about  $40^{\circ}$  or  $50^{\circ}$ ; but we never meddled with the wire or finger at all, but put it on the same circuit as a standard instrument which we took great care of, and then turned the dial till the 150 volts on the dial came to where the needle was, and then the instrument was right for the day. Our rule was that all these instruments were set every morning. One instrument no one touched; we took great pains with it; and that was tested, I think, about once or twice a month with a Thomson galvanometer, a condenser, and one of Mr. Latimer Clark's cells. That instrument was kept perfect, and was compared every morning with every instrument: it took about a quarter of an hour. By that plan we found that the instruments worked uniformly and the lights were kept constant without complaint, which in those days—eighteen months ago—was a very great consideration. I know no other instrument would have given us anything like such a result. We tried nearly every other kind of instrument: they would work for a week or a month, but they all went wrong ultimately. We tried many forms of Cardew voltmeter to increase the sensibility. I went to the length of putting up an instrument 30 feet long, with a pulley and double wire, and tried to get double range with an iron wire, but it was not in any way



practically successful; so we went on and were very thankful for Mr. Gordon's what we got, and always were hoping for a better development of the same thing, which I think Professor Ayrton has now given us. Naturally, in looking at new instruments, we have to think of those who have got to use them commercially and incur responsibilities; because we have to take contracts to keep our electro-motive force absolutely constant within certain limits, and it is a very serious matter if an instrument goes wrong. Though we are all friends here, we are all scientific people, and know the difficulties; yet it is no use telling one's co-directors—business directors—that there is some technical difficulty. They say, "We know nothing about that; you have got us into trouble with our customers;" and we have to get an instrument which is perfect. It seems to me that Professor Ayrton's instrument meets very nearly everything, as far as one can see on short notice like this. There is one very important point in it which Professor Ayrton has not mentioned, and that is that it is the only voltmeter that I have seen—I think not excepting Captain Cardew's, but he will correct me if I am wrong—which has no current in the spring——

Captain CARDEW: Not in my present form.

Mr. J. E. H. GORDON: I was thinking of the instrument of two years ago; and if that is so, there is a possible source of error. Just one point more. Professor Ayrton has pointed out the condition for getting a uniform graduation all round the dials, and a very ingenious condition it is; but that is not always what we want in an engine-room voltmeter. We do not care a bit about it if it is 10 or 15 volts wrong in a low-reading meter; but it is important in a high reading to know whether it is 110 or 111 volts, as an error of one volt at that reading is a serious matter.

I would ask whether by increasing the sag, or any alteration of it, we could make an instrument more sensitive at one particular point—say at the 110-volt point, or somewhere of that sort. That is a question that possibly Professor Ayrton may be able to tell us something about.

I think that is all I have to say, except to express the very great interest that I, in common with all those who have to use



Mr. Gordon. voltmeters practically in considerable numbers, must feel at such a beautiful development of Captain Cardew's principle as Professor Ayrton has brought before us. Nobody who has not had the trouble and annoyance of magnetic voltmeters will know what a blessing it is to be entirely rid of all attractions and repulsions.

Capt.  
Cardew.

Capt. P. CARDEW, R.E.: So far as I can judge, Dr. Fleming's instrument seems a very pretty arrangement of an electro-dynamometer. However, of course, you will expect me to know more about this particular instrument (Professors Ayrton and Perry's). I only saw Professors Ayrton and Perry's very beautiful instruments for the first time to-day, and I had not heard anything about them before; but it is not by any means the first time that the idea of combining Professor Ayrton's very pretty magnifying spring with my principle of using the expansion of a wire has come into my head. I have been many times on the point of writing to him about it, but I never did so, and I was agreeably surprised to find that he has worked it out. Also, if I had known about it, I would have brought up an experimental instrument which I have which is something the same thing as this. It has the wire pulled in the middle, but, as Professor Ayrton remarked, it of course has not got the spring, and it simply was read by means of a lens, the motion of the wire being read against a photographically-divided scale. I have not made much use of it, but the idea was that of pulling the wire in the middle, and thereby getting for a certain expansion of the wire a very much greater motion than you do by pulling at the end. It was suggested by Dr. Muirhead several years ago. The drawback to using it then was that you got the increase of motion at the expense of putting very much more stress on the wire, *i.e.*, if you use anything like the same spring to pull with. Now, of course, that we have got this spring to work on it we can use a very small pull, and have enough power to move our needle, *i.e.*, to move a needle; but these needles are not really what I should call needles for working voltmeters such as Mr. Gordon spoke about.

I have had some experience in the practical working of voltmeters in the very same way that Mr. Gordon has been using them, *i.e.*, putting them up for an engine-driver to work

with; and we not only do that with fixed engines, but with all our portable engines. We take out a traction engine, and the man takes his voltmeter along with him. He sticks it up, and has until lately always stuck it on the engine itself; but we thought that incurred too much vibration, and we gave him a stick to put it up alongside. That is always good enough for him, and we hardly break any lamps.

The old pattern of my instrument had many defects, and, in particular, the wooden box, which I suspect has been the cause of error in Mr. Gordon's instruments, which I know had wooden boxes. I do not know whether they still have.

MR. J. E. H. GORDON: I believe they have.

Captain P. CARDEW, R.E.: It has been rather a source of error.

MR. J. E. H. GORDON: I put brass frames inside the wooden box after I got the instrument.

Captain P. CARDEW: It was really my fault that such instruments were supplied, because I did not think enough about the details. The whole of the mechanism was fixed on to a piece of wood, and naturally it could not be expected that the gear could work very well. I may certainly say that since Mr. Goolden took the voltmeters in hand they have improved very much. The greatest care has been paid to the manufacture of them. This (Professor Ayrton's) form is certainly portable, but as it stands at present it possesses the disadvantage that Mr. Gordon pointed out, of a horizontal face, and so an engine-driver cannot so easily see it. Professor Ayrton has boldly gone in for the use of the fine wire. That was what we used as a safety fuse, but it was a great deal smaller than the wire in these old-pattern instruments. He has been able to use it because he has got a spring which will give him the motion with scarcely any pull at all. This spring can move such a little needle as that, and does move it, very quickly. The finer the wire the more instantaneous is the expansion and contraction, and therefore it is chiefly from the use of the thinner wire that Professor Ayrton gets the rapid motion; and to a small extent also it is owing to having done away with any gearing. The instruments are very beautiful. Professor Ayrton showed

Capt.  
Cardew.

them to me before the meeting. This bicycle-wheel form will be very much improved when it is brought out. There is a good deal too much mass here, but there is a very ingenious idea in it. One point has occurred to me, and that is the question of compensation. You know the wire has to be enclosed in a tube, or something equivalent to a tube, of such metal that no variation of external temperature will cause the zero to vary. If you use an iron tube and put a silver wire in it, stretch it, and put an index at the end, of course when the weather is hot the zero must alter, because the silver will expand much more than the iron. I think there may be some difficulty in arranging for this, and I do not know whether Professor Ayrton has quite thought that out as yet, because the instrument has not come forward commercially.

Another point, perhaps, is as regards the question of radiation, and of the final temperature which will be arrived at when the current has been kept on for a long time: that, again, is made easier by the use of this fine wire. Of course we cannot have the thing getting red-hot, and there must be a certain surface to radiate off the heat. I do not know whether this question of surface has been thought out. There seems sufficient surface in this instrument, but Professor Ayrton talks of putting very high voltage on to a comparatively small surface of metal. However the wire is arranged, if you have so many—say 200—volts with a certain current, you have a certain amount of heat given off, and that must finally come out through the metal surface, and it is very important that that should not get too hot.

Perhaps, as I have not yet described the voltmeter referred to here, I may be allowed to show on the board a standard form which anyone can make for himself. [*Proceeds to do so.*]

Mr. Nalder.

Mr. FRANK NALDER asked if Dr. Fleming had experienced any deleterious effect due to the action of the current at the point. His experience with iridium showed that points of that material wore away very rapidly indeed.

Mr. Preece.

Mr. W. H. PREECE: Might I suggest, Sir, as this subject is one that is very interesting to a great many gentlemen, who may have something to say upon it, and questions to ask, that the

discussion be adjourned until Thursday evening next, when they Mr. Preese. would have a better opportunity of speaking than presents itself at this late hour?

The motion, having been seconded, was agreed to *nem. con.*

Professor W. E. AYRTON: Might I be allowed to make one Professor Ayrton. remark, and one only, to remove the misconception which seems to have arisen from all the specimens of our new voltmeter having been placed on the tables with their dials horizontal? It is quite true, as Mr. Gordon has said, that an engine-driver cannot be expected to keep awake all night looking at a number of instruments which must be used with their dials in a horizontal position, but our instrument can be used equally well vertically or horizontally.

The PRESIDENT: We will now adjourn the discussion until The President. Thursday evening next, when an Extraordinary General Meeting will be held for the purpose.

A ballot took place, at which the following candidates were elected:—

*Members:*

Albert Murray Cross.		Frederick Kinsman.
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*Associates:*

H. Alabaster.		F. A. K. Hounsell.
William Andrews.		Edward Hume Innes.
Henry Barnawall Bristow.		Henry Walter Jenvey.
Frederick Brown.		David Herbert Keeley.
Walter Douglas Campbell.		Henry Byron Moore.
James Coverdale.		Arthur John Morris.
Thomas T. Draper.		William Noble.
Henry Stephenson Edgar.		G. F. L. Preston.
John Davidson Gillan.		John Wright.
Thomas Green.		Julian E. Young.
William Penn Hamilton.		

*Students:*

John Kempe Brydges.		Ernest Bertram Hutchinson.
Edward Gimmingham.		Victor Zingler.
Edward Alfred Horton.		

The meeting then adjourned.

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## ORIGINAL COMMUNICATION.

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ON THE SUPERIORITY OF THE "EARTH-OVERLAP" METHOD IN LOCALISING SMALL FAULTS IN SUBMARINE CABLES WHEN NO LOOP IS AVAILABLE, AS EVIDENCED BY RECENT PRACTICAL RESULTS WITH A FAULT OF 40,000 OHMS IN A CABLE OF 1,140 OHMS CONDUCTOR RESISTANCE.

By A. E. KENNELLY, Associate.

The earth-overlap method of Mr. James Anderson for localising a fault in a single cable, first published by us in *The Electrician* of July 17th, 1885 (vol. 15, page 177), consists of adding resistance to the cable at the end nearer the fault until the latter is brought midway in resistance between the stations, as is proved by finding the same resistance observed from each end when the distant station earths. The resistance then in circuit with the cable represents the added resistance of the Varley loop test.

In the following case a fault of not less than  $40,000\omega$  resistance was localised within  $75\omega$  by tests between ship and shore some  $1,140\omega$  apart. Under the circumstances no other known method would have succeeded in assigning the fault's position with any such accuracy.

This fault was first discovered last April in the Lisbon-Gibraltar cable, on the occasion of the repairing ship's raising and cutting into the cable, about 100 knots from Lisbon, while engaged on other operations with it. Finding the insulation of the section between the ship and Lisbon to be  $100,000\omega$ , twelve knots of cable were spliced on board to that section in order to pay out to the buoyed Gibraltar end and restore communication through the line, so that the ship's testing-room and the Lisbon cable-house were separated by about 112 knots of cable, whose conductor resistance was measured and known to be  $1,142\omega$  within a limit of  $0.5\omega$  possible error.

After the joint between the sea end and the ship's cable was complete, the following were the tests taken at each end, showing

# ORIGINAL COMMUNICATION.

of the cable. The measurements were all with bridge to "false zero." For the purpose of abstrac-

n,

measurement at Lisbon when free at ship is denoted by  $f$ .

"	"	"	earthed	"	"	$e$ .
"	ship	"	free at Lisbon	"	"	$f_L$
"	"	"	earthed	"	"	$e_L$ .

Apparent Time at Ship.	SHORE'S TESTS.										Reading.	E.M.F. Ratio.	Test	Cur. rent	Reading.	Volts.	E.M.F. Ratio.	Test	Reading.	
	E.M.F.	Ratio.	Test	Cur. rent	Reading.	Volts.	E.M.F.	Ratio.	Test	Cur. rent										Reading.
p.m.																				
4.45	30	$\frac{1,000}{100}$	f	...	114,000 <sub>w</sub> falling to 71,000 <sub>w</sub>	45	$\frac{10,000}{1,000}$	f	...	...	...	...	...	60,000 <sub>w</sub> down to 48,000 <sub>w</sub>						
4.50	30	$\frac{1,000}{100}$	e	...	1,125 <sub>w</sub> up to 1,140 <sub>w</sub>	"	"	e	...	...	...	...	...	60,000 <sub>w</sub>						
5.1	30	$\frac{1,000}{100}$	f	Z	87,000 <sub>w</sub> up to 84,000 <sub>w</sub>	45	$\frac{1,000}{1,000}$	f	Z	...	...	...	...	1,140 <sub>w</sub>						
"	"	"	"	C	87,000 <sub>w</sub> down to 49,000 <sub>w</sub>	"	"	"	C	...	...	...	...	1,139 <sub>w</sub>						
5.5	30	$\frac{1,000}{1,000}$	e	C	1,150 <sub>w</sub> down to 1,140 <sub>w</sub>	"	"	e	C	...	...	...	...							
"	"	"	"	Z	1,140 <sub>w</sub>	"	"	"	Z	...	...	...	...							
5.20	...	...	...	...	...	"	...	...	...	...	...	...	...							
"	...	...	...	...	...	"	...	...	...	...	...	...	...							
5.25	...	...	...	...	...	"	...	...	...	...	...	...	...							
"	...	...	...	...	...	"	...	...	...	...	...	...	...							
5.39	30	$\frac{1,000}{1,000}$	e	Z	1,180 <sub>w</sub>	"	"	e	Z	...	...	...	...							
"	"	"	"	C	falling to 1,115 <sub>w</sub>	"	"	"	C	...	...	...	...							

The Blavier results from these figures are—

Shore's 1st test ( $f = 71,000\omega$ ,  $c = 1,125\omega$ ,  $l = 1,142\omega$ ) =  $35\omega$  from shore.

Shore's 2nd test ( $f = 42,000\omega$ ,  $c = 1,140\omega$ ,  $l = 1,142\omega$ ) =  $851\omega$  „

Ship's test ... ( $f = 43,000\omega$ ,  $c = 1,139\omega$ ,  $l = 1,142\omega$ ) =  $357\omega$  „

Mean Blavier distance of fault from shore ...  $475\omega$

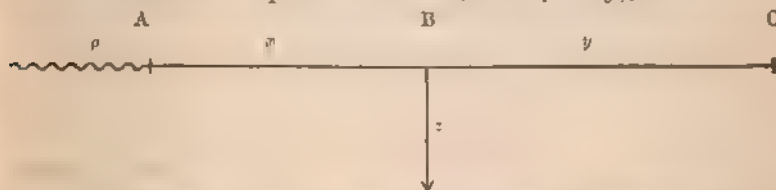
The variation in the Blavier distances here amounted to  $819\omega$ , but it was evident from inspection of the readings that the fault was much nearer shore than ship. Accordingly, in taking the first earth-overlap test, shore added a trial resistance of  $1,300\omega$  to his end of the cable.

Ship and shore now measured and earthed for 30 seconds alternately, the measurements being made at each end with the same battery power (30 Leclanchés), bridge ratio  $\frac{1}{1000}$ , and direction of current to line, with the following results:—

Apparent Time at Ship.	SHIP'S READINGS.		SHORE'S READINGS.	
	Current to Line		Current to Line.	
5 57	Z	missed		
5 57.30	...	...	Z	$2,420\omega$
5 58	C	$2,385\omega$		
5 58.30	...	...	C	$2,420\omega$
5 59	Z	$2,379\omega$		
5 59.30	...	...	Z	$2,420\omega$
6 0	C	$2,363\omega$		
6 0.20	...	...	C	$2,425\omega$
6 1	Z	$2,393\omega$		
Means . . .	...	$2,380\omega$		$2,421.25\omega$

The disparity between ship's and shore's readings here amounting to about  $41\omega$ , showed that  $1,300\omega$  was much too great to effect balance.

In the accompanying figure, A B C represents the cable between shore and ship of resistance  $1,142\omega$  ( $x + y$ ), with a fault



at B of  $40,000\omega$  ( $z$ ). The method consists in adjusting  $p$  at A until

$$\left\{ \frac{p + x + yz}{y + z} \right\} - \left\{ \frac{y + z(p + x)}{p + x + z} \right\} = 0 \quad \dots (1)$$

If we represent the left-hand member of this equation by  $u$ , and differentiate with respect to  $\rho$ , we have

$$\frac{d u}{d \rho} = 1 - \left( \frac{z}{\rho + x + z} \right)^2 \quad \dots \quad (2)$$

and since  $\rho + x = y$  when  $u = 0$  we have, approximately,

$$\frac{d u}{d \rho} = \frac{y + \frac{y z}{y + z}}{y + z} \quad \dots \quad (3)$$

so that if  $\Delta u$  be a small disparity of readings between A and C, and  $\Delta \rho$  the consequent change that must be made in  $\rho$  to effect balance, we have, approximately,

$$\Delta \rho = \frac{\Delta u (y + z)}{y + \frac{y z}{y + z}} \quad \dots \quad (4)$$

In the case above  $\Delta u = 41.25\omega$ , while  $y$  and  $z$  might be assumed at  $1,000\omega$  and  $40,000\omega$  respectively. With these values,

$$\begin{aligned} \Delta \rho &= \frac{41.25 \times 41,000}{1,000 + 976} \\ &= 855\omega. \end{aligned}$$

The reduction to be made in  $\rho$  was thus roughly  $855\omega$ , or from  $1,300$  to  $445\omega$ . Accordingly, in the next trial the added resistance at Lisbon was made  $500\omega$ , and the following results were then obtained:—

Ship's Apparent Time.	SHIP'S READINGS.		SUORA'S READINGS.	
p.m.	Current to Line.		Current to Line	
6 13	Z	1,700 $\omega$ roughly		
6 13.30	...	... ..	Z	1,632 $\omega$
6.14	C	1,623 $\omega$ approximate		
6.14.30	.	.. ..	'	1,622 $\omega$
6.15	Z	1,627 $\omega$ good		
6 15.30	...	... ..	Z	1,627 $\omega$
6.16	C	1,629 $\omega$ good		
6.16.30	...	.. ..	C	1,627 $\omega$ falling to 1,622 $\omega$
6.17	■	1,629 $\omega$		
6.17.30	...	.. ..	Z	1,630 $\omega$
Mean, neglecting 1st reading, 1,627 $\omega$ .				Mean, neglecting 1st reading, 1,625.6 $\omega$ .

The agreement between the ship's and shore's readings was here very close, and the added resistance at the Lisbon end ( $500\omega$ ) was now accepted as balance, especially as, the Gibraltar buoyed end being at this time on board, testing had to be discontinued. Some time later, however, just before making the final splice, the insulation of the cable between the ship and Lisbon was observed by bridge at 8.38 p.m., with 15 volts Z and C  $\frac{10000}{10000}$ , to be  $80,000\omega$  roughly.

The distance of the fault from Lisbon cable-house was therefore assumed to be

1,142	—	500	
	2		= 321 $\omega$

The actual distance of this fault was found by the repairing ship removing it on the 1st October last

to be ... ..	246.6 $\omega$
--------------	----------------

Error of above localisation ... ..	<u>74.4<math>\omega</math></u>
------------------------------------	--------------------------------

The theoretical limit of accuracy of the method in this case is given by equation (4); for supposing the resistance of the fault constant, and the readings to be carried out correctly to the limit of one ohm, their difference  $u$  could be made less than one ohm. If we make  $y$   $1,142 - 247 = 900\omega$  approximately, and assume  $z$  to have been  $40,000\omega$ , and the lowest observed value was  $42,000\omega$ , we have by equation (4)

$$\Delta \rho = \frac{40,900}{900 + 380} = 23\omega;$$

so that the localisation could have been theoretically carried out under these circumstances to  $\frac{23}{2} = 11.5\omega$ .

The errors of measurement, variation of the fault, and incompleteness of balance, increased this limit to nearly  $75\omega$ .

## ABSTRACTS.

### **C. VERNON BOYS—THE RADIO MICROMETER.**

*(Proceedings of the Royal Society, 24th March, 1887.)*

This instrument is an extremely delicate form of thermopile, consisting of a square frame made of one turn of one square centimètre, of which three sides are thin copper wire, and the fourth is a compound bar of antimony and bismuth, each piece being  $5 \times 5 \times \frac{1}{2}$  mm., soldered edge to edge. This frame is supported by a thin rod to which is fastened a mirror, and the whole is hung by a torsion fibre, so that the frame is in the field produced by a powerful magnet with suitable pole-pieces. When radiant energy falls on the centre of the bar the frame is deflected, and the amount of the deflection measures the energy. Adopting suitable dimensions and using a very strong field, an instrument may be made capable of showing a change of temperature of the junction of one thousand-millionth of a degree of heat.

### **Dr. C. ALDER WRIGHT DEVELOPMENT OF VOLTAIC ELECTRICITY BY ATMOSPHERIC OXIDATION.**

*(Proceedings of the Royal Society, 31st March, 1887.)*

When a plate of copper is immersed in ammonia solution, the oxygen of the air is gradually dissolved in the solution and finds its way to the copper plate, with which it unites, the liquid becoming blue. This action may be considerably accelerated by the use of what the author calls an aeration plate, the arrangement constituting really a galvanic cell. The copper plate is entirely immersed in the ammonia solution, and is connected by a wire to a plate of platinum or to a layer of spongy platinum, which is on the surface of the liquid. The platinum condenses the gases of the atmosphere, and from its surface the oxygen makes its way to the copper, a current being set up in the circuit. With spongy platinum and a rather concentrated ammoniacal solution the E.M.F. of the couple was found to be 0.8 volt. Experiments are in progress on other metals which may be substituted for the platinum.

### **Professor J. A. EWING—MAGNETISATION OF IRON IN STRONG FIELDS.**

*(Proceedings of the Royal Society, 24th March, 1887.)*

In experimenting on the intensity of magnetism in iron it has been usual to employ the field produced by the direct action of a solenoidal current, which, however, can scarcely exceed a few hundreds of C.G.S. units. Much more

intense fields may be produced in the space between the pole-pieces of a strong electro-magnet, and such were made use of in the experiments; bobbins of a particular shape of Lowmoor and Swedish wrought iron and cast iron being introduced between the pole-pieces. The magnetic force within the metal differs from the field in the surrounding space by an amount which cannot be estimated without a knowledge of the distribution of free magnetism on the pole-pieces and conical faces of the bobbin. It appears probable that, with the dimensions of the various parts used in the experiments, the magnetic force within the metal is less, but not very greatly less, than the outside and closely neighbouring field. In the absence of any exact knowledge of the magnetic force within the metal, it is interesting to examine the relation of the induction to the outside field. Thus the difference between the induction and the outside field, divided by  $\frac{1}{2}\pi$ , gives a quantity which is probably not much less than the intensity of magnetism. The experiments give no support to the suggestion that there is a maximum of induction. Larger field magnets, with pole-pieces tapering to a narrow neck, should give still higher induction than has yet been obtained.

#### **E. C. RIMINGTON** MODIFICATION OF A METHOD OF MAXWELL'S FOR MEASURING THE COEFFICIENT OF SELF-INDUCTION.

(*Phil. Mag.*, Vol. 24, No. 146, July, 1887, pp. 54-60)

In Maxwell's method (vol. II., sec. 778) the resistance  $D$  possessing self-induction  $L$  is placed in one arm of a Wheatstone bridge, and a condenser in parallel with the resistance  $B$  in the opposite arm; the other two sides ( $A$   $C$ ) of the quadrilateral are ordinary comparison resistances. There is a key in both the galvanometer and battery circuits. An ordinary balance is obtained, putting on the battery before depressing the galvanometer key. Then  $B$  and  $D$  are adjusted until no throw of the galvanometer needle occurs when the galvanometer key is depressed before the battery is put on. In this way a double adjustment, which can only be attained by several consecutive trials, has to be made. Then  $L = KB D$ .

The modification consists in attaching the condenser, not to the terminals of the resistance  $B$ , but to two sliders on that resistance. A permanent balance having been obtained, the sliders are adjusted until there is also a balance when the galvanometer circuit is closed first. If  $r$  is the resistance between the sliders,  $L = K r^2 \frac{D}{B}$ . A revolving commutator may be introduced into both the battery and galvanometer circuits, which, by its revolution, puts on the battery, the galvanometer circuit being closed, breaks the latter or short-circuits the galvanometer and then breaks the battery circuit, afterwards closing the galvanometer circuit. Under proper conditions a telephone may be used in place of a galvanometer.



# JAMES SWINBURNE—PROFESSOR CAREY FOSTER'S METHOD OF MEASURING THE MUTUAL INDUCTION OF TWO COILS.

(Phil. Mag., Vol. 24, No. 146, July, 1887, pp. 85-87.)

The arrangement was made in order to dispense with the use of a ballistic galvanometer in testing the induction through any part of a dynamo. The primary circuit was led through an electro-magnet, representing the dynamo, and through one wire of a double-wound coil, which represented a pair of coils of known mutual induction. This coil was shunted by a wire with sliding contacts. This circuit was made or broken by a switch. A pilot wire round the model electro-magnet was connected in series with the secondary of the double-wound coil and with an ordinary reflecting galvanometer, which was subsequently replaced by a galvanometer with a heavy ballistic needle in order to avoid irregularities in the deflections of the ordinary galvanometer.

## Professor C. NIVEN—SOME METHODS OF DETERMINING AND COMPARING COEFFICIENTS OF SELF-INDUCTION AND MUTUAL INDUCTION.

(Phil. Mag., Vol. 24, No. 148, Sept., 1887, pp. 225-38.)

In the following notes on the several methods with the Wheatstone bridge, the several sides of the quadrilateral are named as follows: the upper sides A D, D C, the lower sides A B, B C, the galvanometer is between D and B, the battery between A and C.

1 To balance the current of self-induction by a condenser. The resistance with self-induction is placed in the arm A D, one terminal of a condenser is joined to A, the other to a slider (P) on A D. Suppose the resistances of A B, B C, C D, D A, A P to be  $a, b, d, c, R$ ; that of the battery B, of the galvanometer G, L the self-induction; C the capacity of the condenser then the equation for the current in the galvanometer branch is

$$y = \frac{E (C R^2 - L) a b}{\{B (a + c) + c (d + b)\} \{(a + c) b + (a + b) G\}}.$$

If  $y = 0$ ,  $L = C R^2$ . If this should involve the use of inconveniently high resistances for R, we may diminish the effect of L by shunting R, the coil itself, by a resistance S.

Then the equation for the galvanometer current becomes

$$y = \frac{E \{C R^2 - L \left( \frac{S}{R + S} \right)^2\} a b}{\{B (a + c) + c (a + b)\} \{(a + c) b + (a + b) G\}}.$$

This remarkable proposition that the effect of a shunt on the self-induction of a coil is to diminish it in the ratio

$$S^2 : (R + S)^2,$$

is of great use in comparing two coils with each other.

2. Instead of placing the condenser in parallel with the resistance having

self-induction, it may be placed in B C. Using the same symbols, the current in the galvanometer circuit becomes

$$y = \frac{E (C R^2 c - L b)}{b \Delta},$$

where  $\Delta$  is the same denominator as above. When  $y = 0$ ,  $C R^2 c - L b = 0$ .

3. *To compare two coefficients of self-induction* The coil L, whose resistance is  $R_1$ , is placed in the arm A D, and a balance for steady currents is obtained by resistances  $r_1$  in A B,  $b$  in B C,  $c$  in C D. The second coil N, whose resistance is  $r_2$ , is then placed in series with  $r_1$  in A B, and is balanced by putting  $R_2$  in series with  $R_1$  in A D. The conditions for this are  $\frac{r_1}{R_1} = \frac{r_2}{R_2} = \frac{b}{c}$ . The point of junction of  $R_1$  and  $R_2$  being called P, and of  $r_1$  and  $r_2$  Q, and the above conditions being fulfilled, no steady current should pass through the galvanometer, whether P and Q are connected by a wire or not. If the second adjustment should throw the first slightly out, the right position of Q, P being kept fixed, may be easily got by making the first part of  $r_2$  and the last part of  $r_1$  parts of the same wire, along which there may be a sliding contact. It is also desirable in most cases to interpolate a resistance into  $R_1$  besides that of the coil itself. A resistance S is then placed in P Q till no kick is given on making contact. The condition of no current is

$$N d - L b \frac{S}{r_1 + R_1 + S} = 0.$$

If  $b = d$ , and therefore  $r_1 = R_1$ ,

$$N = L \cdot 2 \frac{S}{r_1 + S}.$$

4. *To compare the coefficient of mutual induction of two coils with the coefficient of self induction of a third.*—One of the coils of the pair is placed in the battery circuit, and the other is connected to B D as a shunt to the galvanometer. The resistance of this part is  $R$ ; that of the other part is included in the battery. The third coil is placed in A D.

From the equation for the current,

$$y = \frac{E \left\{ \frac{\alpha M (b + d,^2)}{R} - L b^2 \right\}}{\left\{ (a + c + G) b + \alpha G \left( 1 + \frac{b + d}{R} \right) \right\} \{ B (b + d) + J, x + b \}}.$$

we have

$$L b^2 = \frac{\alpha M (b + d)^2}{R}.$$

5. *To express a coefficient of mutual induction in terms of the capacity of a condenser.*—In place of the third coil with self-induction, we may use a condenser in the arm A D in parallel with a resistance X. Then

$$\alpha M b + d^2 = C X^2 R b^2.$$

The differential galvanometer may replace the Wheatstone bridge in any of these methods, but the arrangements would not be clear without diagrams.

### LEDEBOER—MEASUREMENT OF COEFFICIENT OF SELF-INDUCTION.

(*Journal de Physique*, Vol. 6, July, 1887, pp. 320-39.)

There are many difficulties in the way of making practical measurements of the field of a dynamo. The coefficient of self-induction, however, furnishes a means; for in a system consisting of coils containing soft iron wire the flow of induction coincides with the flow of magnetic force. But the flow of force is the product of the strength of the magnetic field by its area while the flow of induction is equal to the product of the coefficient of self-induction by the current. It follows that this latter product is proportional to the strength of the magnetic field.

An account of the experiments undertaken, chiefly in connection with a Gramme dynamo, to verify this view of the proportionality above mentioned; but they would scarcely be intelligible without the numerous curves of results to which frequent reference is made.

### A. LEDUC—HEAT CONDUCTIVITY OF BISMUTH IN A MAGNETIC FIELD, AND DEVIATION OF THE ISOTHERMAL LINES.

(*Journal de Physique*, Vol. 6, Aug., 1887, pp. 378-83.)

One end of a bar of bismuth, enclosed in a glass tube, passes into a steam bath; the bar can be placed in a strong magnetic field by exciting an electro-magnet between the poles of which it lies. Three platinum wires penetrate through the glass tube into the bismuth bar, and by connecting either pair of these wires to a low-resistance galvanometer the difference of potential of the thermo-electric couples thus formed can be measured, and hence the difference of temperature at the junctions, which is proportional to the difference of potential between either pair of wires.

The experiments show that this difference of temperature increases when the electro-magnet is excited, or, in other words, the conductivity of the bismuth is decreased. Similar experiments with one thermo-electric couple, which could be applied at any point on a bar of bismuth, have shown that the temperature at any point is altered on exciting the electro-magnet, and that the isothermal lines are deflected in the same direction as the equipotential lines would be if the heat current were replaced by an electric current.

### C. L. WEBER—CONDUCTIVITY OF AMALGAMS.

(*Annalen der Physik und Chemie*, Vol. 31, Pt. 2, No. 6, 1887, pp. 243-50.)

In former researches the experiments were made on cold amalgams, and consequently it was difficult to determine the conductivity, as parts of the mass might be more fluid than others, and the mercury might not be evenly distributed so as to produce a homogeneous amalgam. By experimenting on amalgams heated to about 260° C., it was certain that they were entirely

fluid and quite homogenous. The amalgams were contained in U tubes, provided with iron electrodes. The resistances were measured on a Wheatstone bridge. The following amalgams were tested: tin, bismuth, lead, and cadmium. The first point to be remarked is that the conductivity of the amalgam is not equal to the mean conductivity of its two components. In all four amalgams the resistance decreases rapidly so long as only a few per cent. of the metal are added to the mercury. In the case of tin and cadmium this decrease of resistance continues, though its curve becomes flatter, and the decrease seems to be proportional to the increase in the percentage composition of the amalgam. In the case of lead and bismuth, on the other hand, after the first rapid decrease of resistance, there is an increase, and the curve rises slowly to a maximum, to fall again very slightly after passing it.

#### V. von LANG—ELECTRO-MOTIVE FORCE OF THE VOLTAIC ARC

(*Annalen der Physik und Chemie*, Vol. 31, Pt. 3, No. 7, 1887, pp. 384-92.)

In a previous abstract (vol. xiv., p. 571) the arrangement has been described by which the resistance of a circuit can be measured whilst a current circulates in it. Former experiments gave the value of 39 volts, and it seemed desirable to repeat them, using copper as well as carbon rods, and also replacing the lamps by compensating resistances.

The new experiments gave the value of 37 volts for the E.M.F. of the arc between two carbons 5 mm. in diameter. With copper rods of the same size the E.M.F. was found to be 27.6 volts, or 0.75 that of carbon, which agrees fairly with the mean value (0.86) found by Edlund. In all these experiments the length of the arc was kept constant; in others, the results of which are tabulated below, the length of arc was varied, and the E.M.F. was calculated from the formula

$$E = a + b l I,$$

in which  $a$  and  $b$  are constants,  $l$  is the length of the arc, and  $I$  the current.

Rods.	$a$ , Volts.	$b$ , Ohms.	$l$ , Mm.	$I$ , Amperes
Carbon	35.07 $\pm$ 1.34	1.82 $\pm$ 0.11	0.4 to 2.5	4.0 to 5.4
Platinum	27.41 $\pm$ 1.16	1.49 $\pm$ 0.19	0.8 „ 3.2	0.3 „ 5.5
Iron ...	25.03 $\pm$ 2.16	0.70 $\pm$ 0.06	0.5 „ 3.6	2.6 „ 5.9
Nickel ...	26.18 $\pm$ 2.95	0.77 $\pm$ 0.13	1.6 „ 7.8	4.5
Copper. .	23.86 $\pm$ 1.33	0.67 $\pm$ 0.04	0.6 „ 7.0	4.1 to 5.2
Silver ...	15.23 $\pm$ 0.45	0.96 $\pm$ 0.05	0.3 „ 7.5	3.7 „ 5.1
Zinc ...	19.86 $\pm$ 2.27	0.56 $\pm$ 0.28	0.5 „ 4.0	2.6 „ 4.8
Cadmium	10.28 $\pm$ 3.38	2.50 $\pm$ 1.27	0.4 „ 1.7	2.5 „ 3.6

#### F. KOHLRAUSCH—MEASUREMENT OF THE SELF-INDUCTION OF A CONDUCTOR BY MEANS OF INDUCED CURRENTS.

(*Annalen der Physik und Chemie*, Vol. 31, No. 4, Pt. 8a, 1887, pp. 594-600.)

Two methods are given—the one with a differential galvanometer, the other with a Wheatstone bridge—as well as the calculation of the equations for each method.

In the case of the differential galvanometer the current impulse of a Weber's magnetic inductor, or of an induction apparatus with an interruptor in the primary circuit, is sent, part, through a resistance  $w$  of self-induction  $L$ , and through one coil of the differential galvanometer, part through an equal resistance, without self-induction, and through the second coil of the galvanometer. The pure resistances are first balanced by means of a constant current. If  $W$  is the resistance and  $E$  the electromotive force of the inductor at any time  $t$ ,  $r$  the resistance of each coil of the galvanometer,  $k$  its constant, the deflection

$$x = \frac{L}{w + r} - \frac{1}{1 + W - w + r} C \int E \cdot dt$$

To determine this integral, an induced current is sent through one half of the galvanometer, after a known large resistance ( $R$ ) has been inserted in the circuit. A deflection  $x$  is produced, and if  $T$  is the time of oscillation of the undamped needle, and  $S$  the damping coefficient,

$$C \int E \cdot dt = x \frac{r}{T} R - W + r S,$$

which, introduced into the above equation, gives for the self-induction the value

$$L = \frac{r}{x} \frac{T}{T} (x - x') \frac{2W + w + r}{E + W - r} \frac{1}{S}.$$

The method with the Wheatstone bridge is very similar, only, in place of the numerator in the above fraction, the resistances of the five branches of the bridge have to be introduced.

## P. KOHLRAUSCH—AN ARRANGEMENT OF RESISTANCE-BOXES TO GIVE VERY LARGE RATIOS WITH EXACTITUDE.

(*Annalen der Physik und Chemie*, F. 4. 31, Pt. 4, No. 8a, 1887, pp. 600-09.)

It is very difficult with the ordinary arrangement of coils in a box of resistances to obtain a measurement correct to, say, one ten-thousandth, if the ratio of the comparison coils is large, such, for instance, as one to one thousand. There is, however, a simple means of arranging resistance ratios of the value  $10, 81, 256, 625, 1,296$ , &c.—that is, of the value  $n^4$ , where  $n$  is a whole number—with such accuracy as is possible in determinations of resistance. Suppose there are  $n$  equal resistances  $r$ , and  $n$  equal resistances  $R$ , and let  $R$  be nearly equal to  $n^3 r$ . If the  $r$  resistances are connected in series, and the  $R$  resistances in parallel, the two totals will be very nearly equal, viz.,  $n r$  and  $\frac{R}{n}$ , which can then be exactly compared by any known method. If the  $r$  resistances are connected in parallel, and the  $R$  resistances in series, the two resulting resistances will be  $\frac{r}{n}$  and  $n R$ . But  $n R = n^4 r (1 + d)$ , and the ratio is  $n^4 (1 + d)$ . The practical outcome of the above considerations is a box of 30 resistance coils arranged in three rows, viz., 10 resistances each of 10,000 ohms, 10 of 100 ohms, and 10 of 1 ohm. Each resistance coil ends in two mercury cups arranged

symmetrically in the ebonite lid. The connections are made by means of bent copper bridges dipping into the mercury, and in this way any arrangement of the resistances in parallel or in series can be readily made. The number of combinations is very large; for instance, the 10 resistances in any one row can be combined in 94 different ways. Some of the advantages of the new arrangement may be cited. There are only three different sizes of coils. In each of the three groups all the coils are alike, and the wire of the same size, and therefore the temperature coefficient may be taken to be the same for all. As the coils are connected in parallel when small resistances are wanted, the radiating surface is larger, and there is less likelihood of the coils heating if traversed by relatively large currents. The use of copper bridge-pieces is preferable to plugs, as a more definite and smaller resistance is introduced by them.

**F. KOHLRAUSCH—CALCULATION OF THE ACTION AT A DISTANCE OF A MAGNET.**

(*Annalen der Physik und Chemie*, Vol 91, Pt. 4, No. 8a, 1897, pp. 609-17.)

The calculations made in accordance with the method first introduced by Gauss are not always sufficiently accurate. The formulæ deduced by the author are:—

$$\text{1st position—} \quad K_1 = 2 \frac{M}{a^3} \left( 1 - \frac{l^2}{a^2} \right)^{-2}$$

$$\text{2nd position—} \quad K_2 = \frac{M}{a^3} \left( 1 + \frac{l^2}{a^2} \right)^{-2}$$

in which  $K_1$  and  $K_2$  are the two forces,  $M$  is the magnetic moment of the magnet,  $a$  is the distance from the centre of the magnet to the pole acted upon, and  $l$  is the distance apart of the poles.

To make use of the above equations in observations, the usual plan of Gauss is followed, in which two deflections ( $\phi_1$  and  $\phi_2$ ) of a magnetometer are obtained for two distances ( $a_1$  and  $a_2$ ). Then, if  $l$  is very small, we have

$$\text{1st position—} \quad \frac{M}{H} = \frac{1}{2} \left\{ \frac{a_1^2 - a_2^2}{\sqrt{\frac{a_1^2}{\tan^2 \phi_1} - \frac{a_2^2}{\tan^2 \phi_2}}} \right\}^2$$

$$\text{2nd position—} \quad \frac{M}{H} = \left\{ \frac{a_1^2 - a_2^2}{\tan^2 \phi_1 - \tan^2 \phi_2} \right\}^{\frac{1}{2}}$$

In using these formulæ for disc magnets, such as magnetised steel mirrors, instead of  $l$  should be put

$$\text{1st position—} \quad l = 0.80 \text{ diameter;}$$

$$\text{2nd position—} \quad l = 0.66 \text{ diameter.}$$



# **A. OBERBECK and J. BERGMANN—MEASUREMENT OF CONDUCTIVITIES BY MEANS OF THE INDUCTION BALANCE.**

(*Annalen der Physik und Chemie*, Vol. 31, Pt. 5, No. 55, 1887, pp. 792-812.)

A great many measurements of conductivity have been made by various physicists, but the resulting values do not agree very well. The discrepancies are no doubt in great part due to the difficulty in obtaining pieces of metal in which the molecular structure is identical. Many of these difficulties may be got over by using Hughes's induction balance for the measurement of conductivities—a use to which it lends itself particularly well, as the measurements are made on thin discs of the metal and not on wires or rods (see infra, Malaret). Moreover, as the currents measured are those due to induction in another circuit, there is none of the great difficulty usually met with in determining the effect of more or less perfect contacts.

The apparatus used by the authors comprised three distinct circuits, one primary and two secondary. In the primary circuit were arranged a battery of 4 to 6 Bunsen cells, a current interruptor, two similar coils (A and B without iron cores, and a coil (C) with a core of small iron rods and wires. In the first secondary circuit were a coil (C') surrounding the primary coil, so that C and C' with the iron core were nothing more than an ordinary Ruhmkorff induction coil, also the two fixed coils of the electro-dynamometer used in the measurements. This secondary circuit, therefore, really served only to produce a fixed field in the electro-dynamometer. In the other secondary circuit—which might be called the measuring circuit—were two coils (A' and B'), placed opposite to the coils A and B respectively in the primary, and forming with them the induction balance, also the swinging coil of the electro-dynamometer. The coils A and B, also A', were fixed, but B' was susceptible of movement parallel to its axis.

Balance having been first obtained with the coils only, one or more metal discs were introduced between the pair of coils A and A', and the balance readjusted by the introduction of a standard disc between the coils B and B'. The method is therefore a zero one, and is compared by the authors to that of weighing with a delicate chemical or assay balance, the experimental body being placed, so to speak, in one scale-pan, the standard weights in the other, and the final measurement being made by observing the number of divisions through which the pointer of the beam is still deflected—the deflection of the pointer in their case being the deflection of the electro-dynamometer.

The standard to which the conductivities were referred was mercury, a disc of this metal being obtained by cementing together thin discs of glass with a circular space between them, much as a liquid object would be mounted on a microscope slide.

The action of the metal disc on the flow of induction depends on its diameter, thickness, and conductivity; and as in all experiments discs of the same diameter were used, the action of the plate or plates between the coils A, A', balanced that of the disc between B, B', when the two products, thickness multiplied by conductivity, were equal. The following table contains the



results obtained by the method described above, and, for the sake of comparison, the values obtained by other experimenters working by other methods, all being referred to mercury as unity.—

	A. Matthiessen and M. v. Bose.	Dénoit.	H. F. Weber.	A. Oberbeck and J. Bergmann.
Copper ... ..	60.36 ( <i>h</i> )	55.86 ( <i>s</i> )	...	54.87
Aluminium ... ..	...	30.66 ( <i>s</i> )	...	30.17
Magnesium ... ..	...	22.57 ( <i>h</i> )	...	18.94
Zinc ... ..	17.52	{ 16.93 ( <i>s</i> ) 16.10 ( <i>h</i> ) }	16.65	15.93
Cadmium ... ..	14.32	13.96 ( <i>h</i> )	13.95	13.77
Tin ... ..	7.66	8.237	9.876	9.045
Lead ... ..	5.02	4.819	5.111	4.688
Antimony ... ..	2.79	...	...	2.459
Bismuth ... ..	0.75	..	0.8004	0.8205

(*h*, = hard, (*s*) = soft.

#### A. OBERBECK—THEORY OF THE INDUCTION BALANCE.

(*Annalen der Physik und Chemie*, Vol. 31, Pt. 5, No. 86, 1887, pp. 812-80.)

The article gives the full mathematical theory of the induction balance as used in the experiments referred to in the preceding abstract.

#### C. BENDER—SALINE SOLUTIONS.

(*Annalen der Physik und Chemie*, Vol. 31, Pt. 5, No. 86, 1887, pp. 872-89.)

His experiments on the specific gravity, coefficient of expansion, and conductivity of solutions of salts and their mixtures, have led the author to the conclusion that some of those salts which are indifferent in their action when mixed, and which he calls "corresponding," have a simple molecular relation to each other. Leaving on one side the experiments relating to specific gravity and expansion, the following are the general results deduced so far as regards resistance and conductivity: Solutions of Na Cl and Li have the molecular ratio of 1 to 1, Na Cl and NH<sub>4</sub> Cl the ratio of 3 to K Cl and  $\frac{1}{2}$  (Ba Cl<sub>2</sub>) also 3 to 4, Na Cl and  $\frac{1}{2}$  (Ba Cl<sub>2</sub>) 1 to 1. The resistance of a mixture of two solutions is almost always less than the arithmetical mean of the resistances of the individual solutions. No similar simple law exists for the conductivity of mixtures.

**WILHELM FEUKERT—EXPLANATION OF WALTENHOFEN'S PHENOMENON OF ABNORMAL MAGNETISATION.**

(*Annalen der Physik und Chemie*, Vol. 32, Pt. 2, No. 10, 1887, pp. 291-97.)

Abnormal magnetisation consists in the formation of the opposite polarity to that which the magnetising current would be expected to produce, and occurs when the magnetising coil is suddenly interrupted, and not when the strength of the current in it is gradually diminished by the successive introduction of more and more resistance. Waltenhofen considers the phenomenon to be due to molecular movements in the iron. G. Wiedemann puts it down to the extra current which is induced on breaking the circuit. The author has tested this view experimentally by suddenly breaking the magnetising circuit of an electro magnet by means of a quick-acting switch, which just before the interruption short-circuits the coil. As the abnormal magnetisation was also noticed in these experiments, in which the extra current did not act, they would appear to support Waltenhofen's view rather than Wiedemann's.

**WASSMUTH and G. A. SCHILLING—EXPERIMENTAL DETERMINATION OF THE WORK OF MAGNETISATION.**

(*Beiblatter*, Vol. 11, Pt. 4, 1887, pp. 278-79)

If soft iron is brought near a magnet from a considerable distance, and then so rapidly removed that the magnetism is not thereby diminished, then more work ( $W$ ) is required for the removal than is given up on the approach ( $L$ ), since the attraction is greater during the removal; the difference  $A = W - L$  is the work of magnetisation. Suppose that the magnetising force  $x$  acts on all parts of the body used, viz., an elongated ellipsoid, in the direction of the axis of rotation with equal force, the work of magnetisation can be calculated per cubic millimètre, if the magnetic moment of the cubic

millimètre is  $m$ . For then, clearly,  $W = \int x m$  and  $L = \int m dx$ , as may be shown by calculation and experiment; hence  $A = \int x m - \int m dx = \int x dm$ .

The experiments were made with iron ellipsoids in an intense magnetic field produced by a large electro-magnet. The forces  $x$  and  $m$  were measured by means of the currents, which were induced in a fixed coil, first with and then without the iron core inside, on reversing the polarity of the magnet; the currents being determined in absolute measure by an earth inductor introduced in the circuit. The experiments were found to fulfil the theoretical considerations.

**V. WIETLISBACH—SELF-INDUCTION IN STRAIGHT STRETCHED WIRES.**

(*Beiblatter*, Vol. 11, Pt. 4, 1887, pp. 284-85.)

The article refers to the experiments of Hughes with the Wheatstone bridge. If  $a, b, c, d$  are the resistances,  $A = -iJ \cdot 2\pi p = -iaJ - \&c.$

the E.M.F. of induction, where  $p$ , &c., are the coefficients of self-induction; then  $\alpha d - \alpha \delta = b c - \beta \gamma$ ,  $\alpha \delta - \alpha d = b \gamma + \beta c$ . In Hughes's experiments  $\gamma = 0$ ,  $\delta = 0$ ; therefore  $\alpha d = b c$ ,  $\alpha d = \beta c$ . The former ratio gives the true resistance, the second gives the coefficients of induction.

If it is assumed with rapidly alternating impulses that the electricity does not distribute itself equally over the whole cross-section of the wire, but is greater towards the surface, then, according to Rayleigh, the resistance of the wire  $W^1 = W \left( 1 + \frac{l^2}{4 W^2} c^2 \mu^2 \pi^2 \right)$ , where  $W$  is the resistance for steady current,  $l$  the length of the wire,  $c$  the number of alternations per second,  $\mu$  the coefficient of magnetisation. Similarly, for very rapid alternations the coefficient of induction is

$$l p^1 = l \left\{ p + \mu \left( \frac{1}{2} - \frac{l^2}{16 W^2} c^2 \mu^2 \pi^2 \right) \right\};$$

and

$$W^1 = \sqrt{\pi c l \mu W}; l p^1 = l \left( p + \sqrt{\frac{\mu W}{4 \pi c l}} \right).$$

For rapid alternations, therefore, the resistance of the wire increases infinitely, and depends on the resistance for steady currents. The coefficient of induction is also variable for rapid alternations.

#### H. LE CHATELIER—MEASUREMENT OF HIGH TEMPERATURES BY THERMOPILES.

(Beiblatter, Vol. 11, Pt. 5, 1887, pp. 351-52.)

The wires generally used are not sufficiently homogeneous to be depended upon. A better plan is to use couples of melted platinum and of a melted alloy of ten per cent. rhodium with platinum, which are very constant. If one junction is kept at  $0^\circ$ , the electro-motive force can only be expressed by a rather complicated formula; if the second junction is at a temperature between  $300^\circ$  and  $1,200^\circ$ , the following formula may be used:— $E = -15 + 0.115 t$ . For a couple formed of pure platinum and palladium, between the temperatures  $0^\circ$  and  $1,500^\circ$ ,  $E = 4.8 t + 0.00073 t^2$ .

#### C. B. CROSS and W. E. SHEPARD—COUNTER E.M.F. OF THE ARC.

(Beiblatter, Vol. 11, Pt. 5, 1887, pp. 373-74.)

Measurements were made of the current in the lamp, and of the E.M.F. as near to the carbon points as possible, the length of the arc being also measured by a micrometer screw. For an arc burning silently the E.M.F. was found to be about 39 volts, for a hissing arc only 15 volts. No material difference was noticed whether the upper or lower carbon was the positive one. The admixture of borax, soda, &c., with the carbon gave an E.M.F. of about 9 or 10 volts. More intense heating of the upper positive carbon, produced by surrounding it with a fire-clay jacket, resulted in an increase of the E.M.F. to 47 volts and 21 volts for the silent and hissing arc respectively. Cooling the upper carbon by a current of water reduced the E.M.F. to 12 volts and 5 volts respectively.

**H. ARON—GALVANIC CELL.**

(Beiblätter, Vol. 11, Pt. 6, 1887, p. 459.)

The cell contains zinc in an alkali. The polarisation is prevented, not by oxide of copper, but by oxide of mercury, which, by dissolving in the alkaline solution, amalgamates the zinc. In order to increase the surface it may be mixed with iron shavings.

**E. KRÜGER—NEW METHOD OF DETERMINING THE VERTICAL INTENSITY OF A MAGNETIC FIELD.**

(Beiblätter, Vol. 11, Pt. 7, 1887, pp. 551-52.)

A brass wire suspends a circular disc of copper in a glass vessel containing a solution of sulphate of copper. Immediately below the suspended disc is a second one, also of copper, supported on a brass rod fixed in the bottom of the glass vessel. Both copper discs have their surfaces insulated, so that a current sent down the brass suspension wire passes radially to the edges of the suspended disc, then through the solution to the edges of the fixed disc. In circuit with the apparatus is a bifilar galvanometer, the convolution area ( $F$ ) of which was determined by comparing its action on the needle of a tangent galvanometer with the action of this latter on the same needle. If the apparatus is brought within the influence of a vertical magnetic force, the horizontal copper disc, being traversed by radial currents, will be rotated.

In the case of the bifilar galvanometer, if  $D_1$  is the directive force of the suspension,  $\phi$  the deflection,  $H$  the horizontal component of the earth's magnetism, then

$$i H = D_1 \frac{1 \sin. \phi}{F}.$$

In the copper-plate arrangement, which is traversed by the same current ( $i$ ), if  $D_2$  is the directive force of the suspension,  $\phi$  the deflection,  $V$  the vertical component of the earth's force,  $r_0$  the inside radius,  $r_1$  the outside radius, and  $l = \frac{1}{2} (r_1 + r_0)$ ,  $d = \frac{1}{2} (r_1 - r_0)$ , then

$$D_2 \cdot 2 \phi = V i l^2 \left( 1 + \frac{d^2}{8 l^2} \right);$$

and from these two equations the ratio of  $H$  to  $V$  can be obtained.

**F. UPPENBORN—METHOD OF CALIBRATING BRIDGE-WIRES.**

(Beiblätter, Vol. 11, Pt. 8, p. 586.)

In order to calibrate the resistance of a wire one metre long exactly to one ohm, a wire of slightly greater resistance is taken, and a shunt is attached to the ends.

**A. ROSEN—SOLUTION OF AN ELECTROSTATIC PROBLEM.**

(Beiblätter, Vol. 11, Pt. 9, p. 643.)

It is not possible to give any extract of the mathematical treatment of the problem, viz., to determine the distribution of the electricity if an electrified

conducting spheres and a dielectric non-conducting sphere are in another dielectric medium, *e.g.*, in air; further, if the dielectric sphere is surrounded by a concentric envelope of another dielectric, or if it is replaced by an infinite quantity of the dielectric which is bounded by the air over an infinite plane. The calculation might be valuable for the determination of the specific inductive capacity, by allowing a dielectric sphere filled with the liquid to be investigated to be attracted by a conducting sphere, and then repeating the experiment with the former sphere empty.

#### A. ROSEN—FRÖLICH'S GENERALISATION OF THE WHEATSTONE BRIDGE.

(*Beiblätter*, Vol. 11, Pt. 9, p. 643.)

If a certain relation exists between the resistances in any network of conductors, and an E.M.F. in a certain conductor (A) produces no current in another conductor (B), then, if any E.M.F. whatsoever are introduced into the network, and the same relation between the resistances is maintained, the current in B will remain the same, whether the conductor A is open or closed.

#### MIALARET—DETERMINATION OF THE ELECTRIC CONDUCTIVITY OF METALLIC WIRES.

(*Bulletin de la Société Internationale des Electriciens*, Vol. 4, June, 1887, pp 331-33.)

There are two ways of determining the conductivity of a wire as referred to a standard. In one, the resistance of a certain length of the wire is measured, and its diameter is exactly determined. We can then compare its resistance with that of one kilomètre of pure copper wire one millimètre in diameter at 0°, which is 20·337 legal ohms or 20·57 B.A. ohms.

The second method is to measure the resistance of a given length of the experimental wire, and the weight of this length, and to effect a comparison with the resistance of one kilomètre of pure copper wire weighing one kilogramme at 0°. In this latter method, which is more especially used in England, it is taken for granted that both the standard wire and the experimental wire have the same specific gravity, which is very frequently not the case, especially with the so-called high-conductivity wires, which owe their high conductivity to their greater specific gravity.

Actual experiments on some wires gave the following conductivities:—

	1st Sample.	2nd Sample.	3rd Sample.
(a) By comparison of diameters ..	102·4	106·7	110·8
(b) By comparison of weights ... ..	101·7	101·2	101·6

It therefore seems desirable to do away with the comparison of weights of equal lengths, and to adopt as the definition of conductivity the inverse ratio of the resistances of the two wires (standard and experimental) of the same length and same diameter at the same temperature.

# LIST OF OTHER ARTICLES

## RELATING TO

# ELECTRICITY AND MAGNETISM,

Appearing in some of the principal Technical Journals during the months of  
JUNE, JULY, AUGUST, SEPTEMBER, and OCTOBER.

(*Philosophical Magazine*, Vol. 23, No. 145, June, 1887.)

- S. BIDWELL**—Electrical Resistance of Vertically-suspended Wires. **Dr. G. FAE**—Variations in the Electrical Resistance of Antimony and Cobalt in the Magnetic Field.

(Vol. 24, No. 146, July, 1887.)

- Dr. J. NIEUWENHUYZEN KRUSEMAN**—Potential of the Electric Field in the Neighbourhood of a Spherical Bowl, Charged or under Influence. **E. H. M. BOSANQUET**—Sequences of Reversals in Magnetisation. **OLIVER HEAVISIDE**—Self-Induction of Wires. **A. P. CHATTOCK**—Magnetic Potentiometer. **Dr. G. A. LIEBIG**—Electrostatic Force necessary to produce Sparks in Air and other Gases.

(Vol. 24, No. 148, September, 1887.)

- E. C. RIMINGTON**—Comparison of Capacities. **A. GRAY**—Elementary Proof of certain Theorems regarding the Steady Flow of Electricity in a Network of Conductors.

(Vol. 24, No. 149, October, 1887.)

- J. BUCHANAN**—Hot Gases as Conductors of Electricity.

(*Annales Telegraphiques*, Vol. 14, May—June, 1887.)

- F. GODFROY**—Adaptation of the Hughes Instrument to Multiple Transmission. **A. DERIES**—Localisation of Faults in Submarine Cables. *Anon.*—New System of Transmission for Telephones and Telegraphs of Maiche and Timmasi.

(*Comptes Rendus*, Vol. 104, No. 23, 6th June, 1887.)

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# JOURNAL

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## SOCIETY OF

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An Extraordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 1st, 1887—  
SIR CHARLES T. BRIGHT, President, in the Chair.

The minutes of the previous meeting were read and approved.

The PRESIDENT invited the meeting to resume the discussion upon the two papers read at the previous meeting, viz., that "On some Instruments for the Measurement of Electro-motive Force and Electrical Power," by Dr. J. A. Fleming, Member, and C. H. Gimmingham, Esq.; and that on "Portable Voltmeters for Measuring Alternating Potential Differences," by Professors W. E. Ayrton, F.R.S., V.P., and John Perry, F.R.S., Member.

The discussion was resumed by

MR. ALEXANDER SIEMENS, who said: I was very glad to hear Dr. Fleming's paper, and to see a new form of the dynamometer which was originally designed by Von Hefner Alteneck, and has been made for some time by our firm; and I have brought here this evening an instrument which represents the latest form in which it has been made. In using it mercury cups are employed, which work perfectly well on land, but are not suitable for use on board ship. The current is indicated proportional to the square roots of the reading limiting the range of the instrument. That has been

Mr.  
Siemens.

Mr.  
Siemens,

partly overcome by putting two coils in all the dynamometers. Still the arrangement was inconvenient; and some time ago Mr. Raworth, who was at that time in our employ, and who was very much engaged in fitting up ships, hit upon a plan which has been modified at our works and the instrument produced which is suitable for use on board ship and has a great range. It is direct-reading—that is, the currents are directly proportional to the deflections—and at the same time there are no movable contacts, which, I am afraid, would be rather a source of trouble in Dr. Fleming's instrument. Dr. Fleming mentions in his paper that it would be bad to have iron in a dynamometer, owing to the variation of the remanent magnetism; but we have overcome that difficulty in this way. A cylinder of iron is pivoted between two fixed points in the usual manner, so as to make the friction as small as possible. It carries two iron arms, which are very thin and flat; and at right angles to these arms a pointer is fixed, the same as in the other dynamometer. At the top is a spring which counteracts the influence of the current. The current enters the instrument through one terminal, goes round the iron cylinder, then passes over and under the two arms, and away to the other terminal. The instrument is so arranged that with a very small current the two thin iron arms are magnetised to their greatest extent; and if the dial of the instrument is inspected at the close of the meeting it will be seen that the divisions at first are proportional to the square root of the angles through which the torsion spring has been turned; but with a current of 10 ampères the arms are quite saturated, and are deflected by larger currents directly in proportion to the current. It can be used in any position, the moving parts being balanced. It has, however, the drawback possessed by all these spring dynamometers: it cannot be left in circuit for a long time without the spring getting a permanent set; it is therefore always advisable, if really accurate readings are desired, that with this instrument a switch be used, so that it can be thrown out of circuit as soon as a reading has been taken.

With regard to determining the volts of an alternate current, we tried to do it in this way: We have here a wattmeter as

constructed by Sir W. Siemens; it is the same form as the dynamometer, only the fixed coils have many convolutions of fine wire, and the direct current goes through the swinging coil only. Measurements are taken by passing the currents through the wattmeter and the dynamometer in succession; then, dividing the one result by the other, the number of volts of the alternating current is arrived at. Anyone will know, of course, that through having so many convolutions there must be a great deal of self-induction; but the instruments (without now going into the details) can be graduated for a certain number of alternations of the current, and they could thus be used in a central station where the machines are always running at nearly the same speed. But the arrangement is complicated, and we have not pursued with it.

The instrument which Professor Ayrton has brought forward as an improvement upon Captain Cardew's is of course very much better adapted to the measurement of alternating currents. I thought it was historically interesting to bring some of the old instruments forward, constructed according to the ideas of Sir William Siemens, in which, by the movement of a lever actuated by a thin strip traversed by the current, more or less resistance could be brought into the circuit and a constant current maintained. The instrument before you has got damaged on its way here, but it was fitted with a lever intended to hold a pencil or some writing arrangement, past the point of which a piece of paper was to be carried, whereby a record could be obtained of the strength of the current and its variations in that particular circuit. I find in the patent of Siemens——

The PRESIDENT: What is the date?

Mr. ALEXANDER SIEMENS: June, 1878. He refers back to a patent of 1877, which was taken out by Mr. Clark as a communication from M. Lontin, who describes a lamp which was regulated by a metal strip, through which the current passes.

The PRESIDENT: What is the date of the patent for the movable coil and the fixed one?

Mr. ALEXANDER SIEMENS: I could not tell you.

The PRESIDENT: Before asking Mr. Gordon for his remarks, I will venture to read the following words from Dr. Fleming's

Mr.  
Siemens.

The  
President.

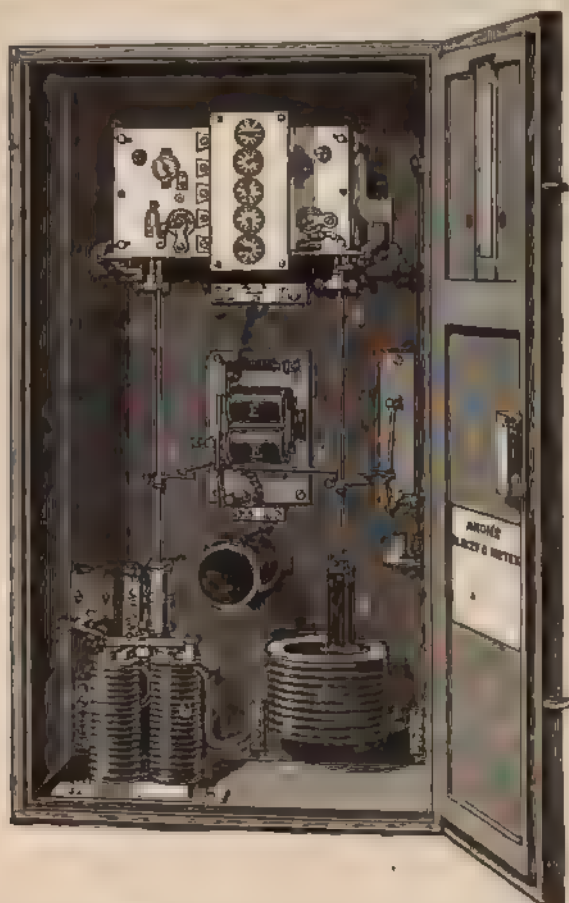
The  
President.

paper: "In these instruments we submit to the criticisms of the Society, the action is wholly based on the fact that, when conductors conveying electric currents are arranged so that one of them is fixed and the other free to move, the force required to hold the movable conductor in any given position in the field of the fixed conductor is proportional to the product of the strengths of the current flowing in these conductors respectively;" and that seems to me very much the same as in Messrs. Siemens' dynamometer immediately in front of me. I wish to remind you that so long as thirty-five years ago my brother and myself, in a patent of 1852 (No. 14,331 of the old law), on page 3, our eighth improvement comprises "a standard galvanometer, or electrometer, for ascertaining the exact amount of force of currents which will give an invariable result;" and on page 8, "The manner in which our eighth improvement may best be carried into effect is as follows:—It is well known that the ordinary needle galvanometer gives no exact test, from the magnetism of the needles continually varying. This we propose to obviate by making a coil to react upon a coil, one coil being fixed, and the other moving from it on a current passing through both; the moving one to be suspended on an axis, and to have a pointer attached to mark the degrees of deflection. Figure 20 will explain the arrangement—A, the fixed coil, composed of a certain given length and size of covered wire; B, coil fixed upon a centre, with pointer attached, contact being made by means of two equal helices of thin wire;" and that is about the same principle adopted in Dr. Fleming's form of apparatus. It is interesting sometimes to compare what electricians were occupying themselves upon with what is being done at present. I will now ask Mr. Gordon if he will favour us with his remarks.

Mr. Gordon.

MR. J. E. H. GORDON. As we are having a discussion about measuring instruments, I thought it would not be inopportune to bring before the notice of the Society, by your kind permission, an instrument which, in the ordinary way of business, has been brought to my notice. The instrument I now show you was made in Germany, and the fact of its not having been previously shown in England must be my apology for bringing it

forward to-night. It is intended to act as a domestic meter, just *Mr. Gordon*, like a gas meter, and is put in the house to register the ampère-hours and indicate the number of Board of Trade units used in a house. A great many such meters have been brought out, and a meter has been a great want in the development of electric lighting that would enable us to get rid of the plan of charging a fixed rental per annum per light instead of charging people according to what



they consumed. The meter before you has been produced by Dr. Aron. Its principle is this: There are two pendulums, one being an ordinary pendulum ( $A^1$ ) with a brass weight, the other ( $A^2$ ) a pendulum of exactly similar length, but its bob consisting of a



Mr. Gordon. permanent steel magnet (B). Underneath the steel magnet bob is a coil of thick wire (C). The instrument on the table is made for 100 ampères, and the coil, or copper rod, is a quarter inch square, wound round and round. When no current is passing, the pendulums travel at exactly equal speed; but when a current passes through the coil the repulsion of the steel magnet bob will accelerate that pendulum and cause it to go faster than the other one. The clockwork attached to the top of the instrument is differential. There are two sets of clockwork, whose speed is regulated by the two pendulums acting on the same set of dials; the dials are of the ordinary multiplying form used for gas meters. When the pendulums go at uniform speed, without a current passing, there is no motion of the wheels, as the one pendulum checks the action of the other; but if the pendulums travel at different speeds motion is produced proportional to the difference of speed—that is to say, to the acceleration. This acceleration is really the sum of all the currents; it is the time integral of all the currents that have passed at each instant throughout the month, or three months, in which the instrument has been working. The instrument is so simple that very little need be said upon it. There are a few mechanical points in it which are of interest. Of course, theoretically, the instrument might just as well be allowed to go on all day when the lamps are not burning and no current is passing, for the pendulums would go at the same speed and there would be no record; but to save the friction and wear and tear the instrument is made self-locking by the catches D<sup>1</sup>, D<sup>2</sup>. As soon as a single lamp is put on, the current passing through the magnet G moves a very light spring which makes contact and, by setting up another current in the magnet E, releases the catches D<sup>1</sup>, D<sup>2</sup>, and allows the pendulums to start. This instrument is of a form intended for actual use, but we are now dealing more with the scientific aspect of the question, and I would remind the meeting that it is a matter of common knowledge that the principle of this instrument was explained by Professors Ayrton and Perry several years ago; and I may say, I think without breach of confidence, that the makers have arranged with those gentlemen for permission to manufacture it. But, as it is, the instrument is now

made by Dr. Aron, of Berlin. I have not tried it myself practically, but Mr. Crompton tells me that he has tried it for some time, that he has made very careful experiments, and has found it accurate and practical. Mr. Gordon

The PRESIDENT: Captain Cardew, have you anything more to say? The President.

Capt. P. CARDEW: Really, I detained the meeting so long last time that to-night I will only show a little experimental instrument which is interesting as being the prototype of that which Professor Ayrton described in his paper, in that it consists of a stretched wire which is pulled in the middle by a spring. There is really nothing in it. The spring is screwed on and forms a pointer; a little magnifying glass is fixed behind the pointer, and by the aid of a photographic film, divided in lines, a scale is given by which readings can be taken. It is only a rough thing; and I did not follow it further because it struck me that it was dangerous to pull the spring except very slightly, and when pulled very slightly error from friction was likely to be caused. Capt. Cardew.

Mr. W. B. ESSON: Regarding the new instruments introduced by Dr. Fleming, I do not clearly see the advantage they possess over forms already in the market, since all that can be done with his voltmeter can be done with a finely wound Siemens dynamometer, and that without taking the current through the spring, which is a very objectionable thing. I should imagine the heating and cooling of the spring would cause its gradual deterioration, so that after a time it would not have the elasticity it at first possessed. Perhaps Dr. Fleming will say something on this point. Mr. Eason

I suppose Dr. Fleming intends his instrument to be used as a laboratory standard only, and not for installation work. For the latter we require an instrument which will indicate from time to time the current or electro-motive force, and not an instrument in which the moving part requires to be balanced by the attendant before an observation is taken. Any instrument of this kind is of very little use in an actual installation.

As regards the wattmeter, I do not think that the profit derived from the sale of such an instrument compensates for the

Mr. Eason. time expended in designing it, for out of some thousands of instruments sold by my firm the wattmeters have barely numbered half-a-dozen. Besides, nobody really wants wattmeters. People do not want to know only the power expended in a circuit, but they want to know how the product of volts and ampères is made up; and although it is quite true, as Dr. Fleming says, that this can be found by using at the same time a wattmeter and a voltmeter, it is equally true that it can be found by using an *ammeter* and a voltmeter.

But, leaving Dr. Fleming's instrument, I have brought down a standard which I am sure will interest all the members of the Society. I may say that this instrument is the largest in creation. It measures 2,500 ampères, and was designed for my firm by Sir W. Thomson. It consists of a movable ring supported above a fixed ring, the movable ring being suspended by two ligaments, each containing 900 wires .1 millimètre in diameter. The current circulates right round the bottom fixed ring, and divides in two in the top movable ring, half going round each way. We have, therefore, attraction between the fixed and movable rings on one side, and repulsion on the other. The force tending to tilt the ring is balanced by moving a weight on the slider, its position when the beam is horizontal indicating the magnitude of the current flowing through the instrument. Mr. Joyce, who is present this evening, and who does most of our calibrating work, would tell you that it is a very difficult instrument to manage. When the beam is nearly balanced, the slightest tug to the cord which moves the sliding weight causes the beam to kick dreadfully, and as a matter of fact it is almost hopeless to balance the current. What we have to do is to put the weight into a certain position and regulate the current, increasing or reducing it very gradually until we get the beam balanced, noting at that instant the deflection of the instrument being calibrated. Of course a spring instrument is much easier to work, because the spring can be made to balance the current, or the current can be graduated to balance the spring; but springs lack the constancy of weights.

Coming to the Cardew voltmeter, I have brought down, for

the purpose of comparison, an instrument of our latest form. Mr. Eason.  
This instrument, which can be examined at the close of the discussion, is one extremely easy to wire. There are four wires in it, but two only are used to indicate by their expansion the E.M.F.; the other two are simply idle wires introduced as resistance. This instrument absorbs some 36 or 40 watts, and from the figures which Professor Ayrton gave me last time I think his absorbs the same amount. It will be seen, however, that the radiating surface of Professor Ayrton's instrument is extremely small compared with the radiating surface of ours, and I imagine the temperature must be high. I should like to ask Professor Ayrton whether his instrument has been kept to the full voltage for any considerable length of time. I do not know whether it will get to 300° F. or not, but if it does I imagine that the heating and cooling of the spring would affect its permanence. Perhaps Professor Ayrton will say something about it.

Professor Ayrton has said that in his instrument there has to be a certain initial sag given to the wire: that means, I suppose, a certain amount of slack. In re-wiring the instrument that amount of sag must be left. I want to know how that slack is adjusted. I suppose there is an adjustment so that when the instrument is re-wired one can be sure to have the same amount of sag as before. It is very important that instruments of this class be easily re-wired. There should be no necessity to send them to the maker for this purpose. I have never wired a Cardew voltmeter, but I never looked upon it as a very formidable thing until I saw Professor Ayrton's instruments. I should not like to have to re-wire one of those. I think, in the matter of simplicity of wiring, the older form of instrument has certainly the advantage.

Though expressing an opinion in favour of the older form of instrument for the measurement of E.M.F.'s of 120 volts and upwards, I wish to add that the instrument of Professors Ayrton and Perry is the only instrument which will measure satisfactorily *small* alternating differences of potentials, for the *observation of which it was originally designed.*

Mr.  
Evershed.

Mr. SYDNEY EVERSHED: I like the appearance of Dr. Fleming's instruments, although, like Captain Cardew, I certainly expected them to be on a much larger scale. Their small size is not a disadvantage, however, if they are to be carried about to different installations. Dr. Fleming speaks of their being direct-reading. I think that is a misuse of the term. "Zero-reading" instrument seems to me a more correct expression; and an instrument which is calibrated all round the scale, and which indicates the quantity to be measured without any adjustment on the part of the observer, is a "direct-reading" instrument.

No one has said anything about the temperature errors of springs. I have not had much experience with them, but I expect they have a kind of temperature coefficient; probably Dr. Fleming can tell us whether they have or no. I am rather surprised to find the authors of the paper have used platinoid wire for their voltmeters, because, as far as my experience goes, if you wind a voltmeter of that type with copper, and put a platinoid resistance in series with it, you spend less power than if you wind the instrument solely with platinoid. Voltmeters can be compensated for temperature errors by several methods of winding, described fully by Mr. Swinburne in a paper read before the British Association this year. Dynamometers like those before us are easily compensated by shunting the movable coil with a copper resistance, and they will then read absolutely correctly to one part in a thousand at all temperatures. As an example, if the fixed coils are ten times the resistance of the movable coils, both being of German silver, you want the copper shunt to be three times the resistance of the movable coil, and the power wasted is really very small.

I had intended to say something about the compensation of instruments in general for temperature. It is a subject which has been brought before the scientific public by Mr. Swinburne, and I have been working at it practically, but my instruments are not yet ready. I hope, however, early next year to bring before this Society a voltmeter which is not affected by temperature to *an extent* greater than one part in a thousand over a range of

50° C. Moreover, it can be left on, as the readings are unaffected Mr.  
Evershed. by the power spent in the instrument.

Coming to Professors Ayrton and Perry's new low-reading voltmeter, it seems to me that it is admirably adapted for testing secondary cells. There is a distinct want for an instrument to indicate up to about 5 volts. There are many inquiries for such an instrument, but up to the present nothing really satisfactory has appeared.

Professors Ayrton and Perry object to gearing. Well, if you want an instrument which can be knocked about by an engine-driver, or, in fact, anyone else, you had better use gearing. The use of it enables us to do away with the objectionable silk which was passed round the spindle. There is no objection to it provided you get good wheels. The perfect curves obtained from the present type of Cardew voltmeters show us that the wheels are practically perfect.

The amount of power spent was referred to by Mr. Esson, but I fancy he was simply thinking of its effect on the spring. I have already asked Dr. Fleming about the effect of temperature on springs; but there is another point that occurs to me. In the instrument reading up to, I think, 10 volts, the power appears to me to be very small, and an instrument of that size may be safely left on; but when you come to the "bicycle" form the power spent becomes considerable. There is an instrument on the table which I think reads up to 170 volts.

Professor AYRTON: 30 volts.

Mr. SYDNEY EVERSLED: I am referring to one on the table. The power spent in it is something like 50 watts, and the surface of the case is not enough to radiate that energy; it would get very hot. In the ordinary pattern of Cardew voltmeter we find 5 to 7 square inches of surface allowed per watt, and the instruments get quite hot enough. I am quite sure the instrument on the table could not be left on.

There is a very curious point I have come across in making Cardew voltmeters with respect to the energy radiated by fine wires and by thick wires. If you take a given length of two wires of two different diameters, say one 3.5 mm. and another 1.4 mm.,



Mr.  
Evershed.

and pass currents through them so that they are raised to the same temperature (taking expansion as a measure of the temperature; one need not know what that temperature is—it may be something like  $200^{\circ}$  C.), one would fancy that a fine wire required less power than a thick one to keep it at the given temperature. I certainly expected that; and Captain Cardew urged me to construct voltmeters of fine wire, because they would take less power. I tried them, and found they did not. In the first experiment I made, the fine wire positively took slightly more power to raise it to the given temperature than the thick wire. I repeated the experiment very carefully. I have taken one voltmeter, so that I did not use different gearing. I used the same wheels and the same part of the wheels, and made the strain on the wires proportional to their breaking weights, so that the wires were under the same conditions. I strung the voltmeter with three different sizes of platinum-silver wire—1.4, 2.5, and 3.5 mm. (the wire is apparently about the same composition, because its resistance agrees with its diameter)—and on each occasion I raised the wire to  $280^{\circ}$  deflection on the dial, which I make correspond roughly to  $200^{\circ}$  C.; and the results were that the watts spent in the 1.4 mm. wire were 38, in the 2.5 mm. wire 36.9, and in the 3.5 mm. wire 32.2. Now I cannot at present say that those figures are exactly right, because one does not know the exact composition of the wire—it is absolutely essential that one should know that—but it is quite clear that the power does not diminish as you diminish the size of the wire. Captain Cardew objected to that because, he said, the 2.5 mm. wire fused at about double the current taken by the 1.4 mm.; but that is not the point at all: the point is that the power spent ought to be greater with the fine wire at a given temperature. For instance, I tried the 1.4 mm. wire, and it fused at .45 ampères, while the 2.5 mm. wire fused at .75 ampères; but even in this case the power was greater for the fine wire. There is really no doubt that those experiments give a correct representation of the facts. As for a correct explanation I am completely in the dark.

The use of platinum-silver in the Cardew voltmeter seems to have been a very happy idea on Captain Cardew's part. I have



had a platinum-silver wire, six feet long, hanging up for six months, strained to half its breaking weight the whole of that time by a weight, and with gearing applied to it so that I could very easily detect a difference in its length of 1 in 100,000. I first stretched the wire by running a current through it, as we always do for our voltmeters: it stretched a little, but in about a week it came back to its old length before it was stretched, and there it has remained for six months, which shows that we may rely upon a platinum-silver wire remaining the same length; the changes observed in the zero of Cardew voltmeters being due solely to the springs used.

The PRESIDENT: I will ask Mr. Swinburne for his remarks, and remind him that as several others wish to speak, and as the replies must be given to-night, it is necessary that observations be condensed as much as possible.

Mr. J. SWINBURNE: Electrical engineers must be glad that Messrs. Fleming and Gimingham have brought out an instrument, for we may be sure that great care will be taken in calibrating it. Though a few makers calibrate their instruments carefully, inaccurate calibration is a much more common fault in the various instruments in the market than bad design.

Those who have spent much time designing instruments in which the force between two wires carrying currents is measured, must have come across the difficulty of arranging the coils so as to get a large force with little waste of energy in a zero instrument without making it in unstable equilibrium. For instance, two fixed coils of the same size, with a common axis, are in unstable equilibrium when the current attains a certain value. One cannot but admire Messrs. Fleming and Gimingham's ingenious way of getting a large force with stable equilibrium. Their arrangement also prevents errors from the instrument being out of balance, as the induction is practically radial. I presume the diagrams are incorrectly drawn, and that the direction of the current on one side is really the reverse of that shown. The instrument is then unaffected by neighbouring dynamos, being astatic. Instruments with one moving coil have to be read twice with the current in different directions under such circumstances, *the mean between the readings being taken.*

Mr.  
Ewinburne

I do not think there is any objection to bringing the electricity in by the spring; the surface is so large, and the current-density so small, that there is no perceptible heating. Taking it out again by a metallic point seems much more objectionable. I have tried making contacts through points on which the moving coil rocks, but found that the instrument soon lost its sensitiveness. I have also used two springs: the electricity came in by one and went out by the other. This works very well, but is difficult to apply to zero instruments. The temperature error cannot be large, as the cheapest watch is a very accurate instrument compared with a voltmeter. A temperature of  $100^{\circ}$  does not seem to weaken the springs used in safety valves and indicators, so there is no reason to suppose voltmeter springs would vary. I am glad to see the prejudice against springs dying out, and hope to see that against permanent magnets follow. Messrs. Fleming and Gimmingham do not say whether they intend their instrument for alternating currents; if so, there will be no error due to the mutual induction of the coils when used as a wattmeter. This is absent from Siemens'.

A method of compensating for wattmeter errors was also explained in my British Association paper already referred to; this can be easily applied to the instrument before us.

Whenever I can possibly do so, I "go for" Professor Ayrton, partly on principle, but chiefly because I think he likes it. Tonight I cannot do so. I very much regret it. Everybody has coveted Captain Cardew's hot wire, and each of us has wished he had invented Professors Ayrton and Perry's beautiful helical spring. It is exactly what one wants in designing all sorts of instruments. But if Professors Ayrton and Perry wish to raise this beautiful instrument above the level of a voltmeter, I think they must compensate it. Captain Cardew's voltmeter has to be compensated by making the case with the same expansion coefficient as the platinum-silver wire; as the case cannot be made of platinum-silver, it is made of brass and iron. It has also to be corrected for alterations of resistance with varying external temperatures.

In measuring alternating currents a voltmeter that measures

the root of the mean square is of no use on the primary circuits. Mr.  
Swinburne.  
 Secondary circuits, again, nearly always feed incandescent lamps. People do not seem to realise that lamps do not behave like resistance; they flicker to some extent, especially if the carbons are thin. This makes them apparently run at a higher efficiency and break sooner. A fine-wire voltmeter such as Professors Ayrton and Perry's must flicker to some extent also. In using a hot-wire instrument for alternating-current work several assumptions are made which do not seem altogether warrantable. First, it is assumed that the wire radiates heat uniformly without flickering. Second, that the current-density is uniform throughout the section of the wire, or that it corresponds to the current-density with a direct current, which is not quite uniform, because the inside of the wire is hotter than the outside. It must also be remembered that the mutual induction of the electricity streams affect the current-density with alternating currents. Third, that the temperature throughout the cross-section is the same as with the corresponding direct current. Fourth, that the instrument has no appreciable self-induction. I have not calculated it out, but it seems as if a very thin wire might cause a sensible error; and I would like to know if anyone has calculated the self-induction of a Cardew and of a Siemens instrument, or measured it.

Lieut.-Colonel R. Y. ARMSTRONG, R.E.: I would ask to what extent Professor Ayrton's instrument is affected by external changes of temperature; and I should be also glad if Dr. Fleming would give us information on the same point. Further, I would ask to what extent these instruments can be used as standard instruments, as I do not think anything has been said as to the degree of accuracy with which they would work. Lieut.-Col.  
Armstrong.

The PRESIDENT: Mr. Swinburne referred to Mr. Gimmingham, and as that gentleman is present I will ask if he has any remarks to make in addition to what we have heard from Dr. Fleming about his instrument, as he was absent at the reading of the paper. The  
President.

Mr. C. H. GIMMINGHAM: I am much obliged for the opportunity, but I have carefully read the paper that Dr. Fleming Mr.  
Gimmingham

Mr.  
Gillingham

has arranged, and quite coincide with all that he said, and I really have nothing to add to it.

Capt.  
Sankey.

Captain H. R. SANKEY, R.E.: I would simply ask Professor Ayrton what kind of fuse he would put on his instruments. I believe the wire in them is .0004 mm. in diameter, which is the finest that can be made.

Mr.  
Spagnoletti.

Mr. C. E. SPAGNOLETTI: I would ask to what extent the instruments have been practically used.

Mr.  
Gumpel.

Mr. C. G. GÜMPEL: There is one point to which I should like to call attention, as I find it has only been slightly touched upon by Mr. Esson; but before I do so I should like to support our Chairman in his introductory remarks at the last meeting with regard to two papers being read and discussed together, which I certainly believe is a habit that is not advisable. Suppose you go to an electrician and ask him for an ammeter, and while you discuss with him the merit of his ammeter he were to tell you about wonderful voltmeters he had, then turn to ergmeters, and so go over the whole range of measuring instruments, turning from one to another, and occasionally referring to the ammeter, we would soon tell that electrician that it was no use talking about other things when an ammeter only was wanted. So to-night speakers have wandered from one thing to another and gone back to the first again; and I believe it is exactly the same as I often find in discussing philosophical questions—that there is great difficulty in following controversialists who pass from subject to subject: nothing is more beneficial than to thrash out one paper, or instrument, before going to another. As one of those who voted in favour of the President's proposition last meeting, I was sorry to find that the papers were mixed up; we never get clearly at the ideas of the speakers against, or for, one or the other—at least, not so clearly as we should like to hear them.

I should like to make a few remarks about Dr. Fleming's wattmeter and his voltmeter. It strikes me that, generally speaking, it is not an instrument of the future in the workshop, for the simple reason that it does not avoid the disadvantages

of the Siemens electro-dynamometer: it is not free from self-induction, it must be levelled, its contacts are very doubtful; and it is much more an instrument of the laboratory than of the workshop. Other speakers have dwelt upon various points with regard to it, and I will not dilate upon them more.

Since we have been wandering from one subject to another, I take the liberty of doing the same, and refer to Dr. Aron's ampère-meter. When Professor Gordon set the two pendulums going it reminded me of the Emperor Charles V., who found the greatest possible difficulty in making a number of pendulums oscillate in equal time; and anyone who has attempted to adjust a pendulum for exact time-keeping, such as is insisted upon by Sir Edmund Beckett Dennison in his book (as I have found to my cost), will be aware of the great difficulty of doing so on account of the extremely fine adjustment required. These two pendulums in Dr. Aron's instrument, unless they are very closely adjusted, will show a certain difference; and that, to my mind, makes that instrument doubtful in regard to its accurate indications.

I now come to the point I wish chiefly to arrive at, which is to ask Professor Ayrton whether his experience has led him to see what influence the repeated tension and contraction, or expansion and contraction, of the platinum wire against the spring has had upon the accuracy of the instrument. I should think that a thin wire—of, I understood, .04 mm. in diameter—would stretch by being constantly in tension with the spring; and as it elongates no doubt it becomes weaker, but still has such a pull upon it that it will never allow the wire to get back to its original condition. The wire itself would alter its molecular structure, and so ultimately make it less accurate in its indications. I mentioned this to Professor Gordon previous to his remarks last meeting, and he answered by saying that the voltmeters were adjusted every day.

Mr. J. E. H. GORDON: The adjustment was extremely small.

Mr. C. G. GÜMPPEL: Yes; but still sufficient to necessitate it. It has been somewhat modified in Professor Ayrton's instrument,



Mr.  
Gumpel.

though I do not know how it is possible to avoid it except by an adjustment of the wire and the spring.

Dr.  
Fleming.

Dr. J. A. FLEMING, M.A., in reply, said: In the first place, on behalf of Mr. Gimmingham and myself, I beg to return to the Society our thanks for the kind way in which our paper has been received, and for the useful criticisms which have been made by different speakers upon these instruments. As far as possible I will endeavour to take, very shortly, in order the remarks of those who have spoken.

I agree with Mr. Gordon's remark that one of the most pressing wants is really a good engine-room voltmeter—one which can be seen across a large engine-room, and which gives a good indication for a pressure of 1 volt in the neighbourhood of the pressure which you are using. Our intention in designing this voltmeter was not to produce an engine-room voltmeter, strictly speaking; and therefore, that being so, I cut away the ground of some part of Mr. Gordon's criticism, in which he stated that an instrument over which an engineer or dynamo attendant would have to stoop would be objectionable.

I did not in the paper give the dimensions of the spring, but they are as follow:—The spring is formed of steel wire of rectangular section, the dimensions being 6 mils. by 8 mils., and the cross-section of the spring is therefore 48 square mils. When the instruments are taking about 1-15th or 1-20th of an ampère, the current-density in the spring amounts to 1,000 ampères per square inch, which is quite a safe current-density for a wire of such small section. The spring itself will not become heated above a temperature of 60° C. if the current is left on.

Turning to the instrument which Mr. Alexander Siemens has presented to us, and which is upon the table, there is no doubt that the instruments that we have brought forward are precisely of the same type; the principle of the electro-dynamometer is of course not new. The first person to arrange an electro-dynamometer was Weber, and the original instrument is described in his work, "Electro-dynamische Maasbestimmungen," published in 1846.

I am very glad to see upon the table that instrument which Mr. Esson has had placed there of Sir Wm. Thomson's. I had a letter from him only the other day describing that instrument to me, and telling me that it differed in some details from the instruments which he brought before the British Association last meeting. We have in our laboratory two of Sir W. Thomson's standard current meters of that type, which we use constantly as standards. They are certainly very beautiful instruments, and could not but be so when we recollect the immense ingenuity and wonderful theoretical knowledge which Sir W. Thomson brings to bear upon all that he does. But at the same time it must be admitted that the instruments themselves, in their present form, are not very portable standards. We use our standard instruments for making comparisons of other instruments, and they are never moved from the place where they are set up.

Dr.  
Fleming.

With respect to the effect of the current on the elasticity of the spring. Sometimes objections which seem formidable in anticipation do not turn out to be very great in practice. We have tested the instrument by keeping the current on for a long time, and we have not found, when the instrument has been kept in circuit for some time, that there is any alteration of reading due to change in the elasticity of the spring within the limits which the instrument is intended to read.

I was very glad to hear Mr. Evershed's remarks on the instruments, because I know that he has paid great attention practically to the manufacture of electrical measuring instruments; and I agree with his remarks that the instruments, by being wound partly with copper and partly with platinoid, might be made to take less power than they do if they are wound entirely with German silver wire. But one reason which makes us prefer German silver to one entirely of copper is that it is then unnecessary, in practice, to provide people with a variation coefficient. If a voltmeter is wound entirely of copper, as many instruments are, then, in spite of the fact that the coefficient of variation may be given inside the case, people *will* take out the voltmeter and use it without applying any coefficient at all.



Dr.  
Fleming.

Let us see what would be the effect if instruments wound with copper alone were used by a not very highly trained person who simply disregarded the coefficient. Suppose the instruments are correct at  $15^{\circ}$  C., and are marked to that effect: if they are used in an engine-room at  $35^{\circ}$  C., and the electro-motive force also measured by them at a point in the open air which is at  $5^{\circ}$  C., the voltmeter would read 8 per cent. too low in the engine-room and 4 per cent. too high outside; thus the reading, if uncorrected, would be 92 instead, say, of 100 inside, and 101 instead, say, of 97 outside; in other words, it would just reverse the direction of fall of pressure. Of course anyone would immediately say the correction for temperature must be applied; but then the attendant may forget to do so.

Amongst other things Mr. Swinburne mentioned the fact that the mutual induction between the coils increases practically the self-induction of the instrument as a whole, and that therefore of course it is not available for alternate currents. We did not put the instrument forward as a means for measuring alternate currents; we were perfectly well aware that an instrument of this type was not available for the purpose. As a matter of fact, when the alternations are not very great the instrument reads the same with alternate as with direct currents; but, as we all know perfectly well, an instrument calibrated for one periodicity of alternate current would be of no use for general purposes.

Colonel Armstrong asked if the instruments are of use as standards, and to what degree of accuracy they can be depended upon. The great object which we had in designing these instruments was to make a portable standard which should be free from the objections which had undoubtedly existed in instruments in which iron was used. It is possible to make a reading with a 100-volt instrument to about  $\frac{1}{4}$  volt, or perhaps less with some practice, and the instruments are intended to be sensible to within one part in 400 or 500 in that part of the scale in which presumably they would be most used.

Mr. Spagnoletti asked to what extent the instruments had been practically used. They have not up to the present been in the hands of the public generally, but they have been very

thoroughly tested, and we ourselves are satisfied that we have obtained a convenient and portable form of electro-dynamometer. The old proverb that "the proof of the pudding is in the eating" must apply not only to puddings but to voltmeters, and we can only submit instruments to the criticism of those who are in the habit of using voltmeters: by their verdict we shall be prepared to stand or fall.

Let me say one word upon the instruments which Professors Ayrton and Perry have brought before us. A Cardew instrument with an uncompensated case certainly requires a very large temperature correction, and it may be a very large temperature error. The compensation of the case seems to me a very important point indeed, and although I may not at the present moment have fully understood Professors Ayrton and Perry's instrument, yet I have not been able to see exactly how this compensation is intended to be effected. I should like to ask how that difficulty is overcome. There is one way in which I think all temperature errors may be got rid of in the Cardew instrument. Since last week we have made experiments in our laboratory to see if a differential principle could not be applied to the measurement of the sag of a wire. If, instead of attempting to measure the sag of one wire, we measure the difference of sag of two wires, through one of which a current passes, and through one of which it does not, an instrument is obtained which is independent of the change of temperature as a whole, but which measures the temperature of the wire through which a current is passing. Our experimental instrument is like this. Here is one wire stretched, and above that another wire is stretched; we will call one the hot wire, the other the cold wire. A thread is attached to the hot wire which passes round a pulley and returns to the cold wire. If both wires sag together the pulley is not rotated at all, but if one wire sags more than the other then the pulley is rotated at a certain angle, which depends upon the sag of the wire through which the current is passing. Experiments have shown us, I think, that it will be possible to arrange an instrument in that way which will be independent of the change of temperature of the instrument as a whole. The only

Dr.  
Fleming.

Dr.  
Fleming.

difficulty that seems to me to occur in measuring the sag is that, in order to make it much more advantageous to measure the sag than to measure the actual increase of length, you must of course have a small sag; in other words, the wire must be much pulled. When very thin wire is used there is great danger that the wire may be permanently elongated if more than a very small stress is applied in the direction of the sag.

I think I have covered most of the ground that was raised in the discussion, and I will only, in conclusion, once more thank the Society for affording us the opportunity of bringing these instruments before them.

Professor  
Ayrton.

Professor W. E. AYRTON, in reply, said: We did not refer to the late Sir William Siemens' very ingenious current regulator—although one of us was present at the Royal Society when it was shown there in 1879—because, as we were employing the Cardew principle in our improved alternating-current portable voltmeter, we did not wish even to appear to detract from the merit of Captain Cardew's principle. At the same time it must not be forgotten that Sir William Siemens not only described a current regulator depending on the expansion of a metal by heat, but also a current meter based on the heating property of the current. M. Hospitalier was also working at the same time at the same subject, and his name must not be overlooked in the history of these instruments. As to the voltmeter which Captain Cardew has shown this evening in which the volts are measured by the sag of a wire measured with a microscope, it was not until last Thursday (as I then mentioned) that we were aware that Captain Cardew had thought of employing the variation of the sag, nor was it until this evening that we saw this form of his instrument. However, I am merely repeating his words in saying that without the employment of the device of our magnifying spring the variation of the sag could not be utilised in the construction of a commercial instrument.

I agree with the criticism that has been made that the ingenious form of electro-dynamometer exhibited by Messrs. Fleming and Gillingham cannot be properly called "direct-reading." I think we ought to keep the name "direct-reading" to an instru-

ment which indicates the thing measured exactly in the same way that a clock indicates the time without one having to touch it: one ought to be able to see for some distance what one wants to know, without going near the instrument.

As to Mr. Esson's remark about the use of a wattmeter for direct currents, I am rather inclined to agree with him. A wattmeter presupposes some difficulty in the power of the observer to perform simple multiplication. Well, whatever that difficulty may have been with electrical engineers in the early days, it has certainly disappeared, and it is possible for them to perform a simple sum of multiplication. I have, I think, a right to criticise the wattmeter, because the first wattmeters made in this country—or "electric-power meters," as they were then called—were constructed by my colleague and myself, and the way in which they were made public is rather interesting. Two papers were down for reading at the semi-centenary meeting of the British Association at York in 1881—one by Sir W. Thomson, and one by my colleague and myself. As a matter of fact, in the order of the papers to be read, as printed on the list, ours came first, and Sir W. Thomson's came second; but Sir William asked us, as he had to go to a committee meeting, whether we had any objection to his reading his paper first. Of course we said "No." He read his paper, and he described, without showing any instrument, the principle of a wattmeter. We were able to say when he, as chairman of the sectional meeting, called on us immediately afterwards to read our paper, that he had already read it, and that our instrument on the table was the instrument which illustrated the paper he had read. He, in his jocular way, said that he had priority of announcement because he had read his paper first (although, as a matter of fact, in the order of papers his came second). We replied, also in a jocular manner, that we had constructed the instrument, whereas he had only constructed the idea.

I have not had any experience of Sir W. Thomson's current meter on the table before us, but I should think that, like all his instruments, it is extremely beautiful and extremely accurate; but such an instrument as *this* aims at attaining a totally different

Professor  
Ayrton

result from the sort of instruments that we have from time to time brought before this Society. In fact, I think there is the same sort of difference between Sir William's instrument and some of the others, like those on the table, as there is between the astronomical clock at Greenwich and the watch one has in one's pocket. There is no doubt that the astronomical clock is the better of the two, but it cost the nation a good deal more than I could afford to pay for my watch. It is not only that, but I think, as Mr. Esson has said, it is probable that, whatever its name may be, the instrument is really a "man-tester;" that is to say, it tests your power to use it more than any electrical quantity. Far be it from me to disparage any instrument that Sir W. Thomson has brought forward: my intense admiration for his unique originality would make any such disparagement sacrilegious in my eyes. He has designed and, what is more, constructed many instruments to enable us to do that which we cannot do any other way, and for that we must be deeply grateful; but at the same time I have been led to feel for many years that it was quite as much my power to use the instrument as any other physical quantity that was being measured when I was employing one of Sir William Thomson's meters.

A question was asked as to the exact sag that had to be put in the wires in the new form of voltmeter. I may mention that there are two adjustments which enable us to get the sag exactly right when re-wiring is required. In the first place, the wires can be stretched more or less by little eccentrics; and more or less tension can be given to the spring by means of an adjustable screw (shown in the various drawings of the voltmeter); and by the use of these two adjustments, if we use the same gauge of wire, we can reproduce with very considerable accuracy the previous sensibility. As to the difficulty of wiring, Mr. Esson made a sporting offer as to what he would be willing to do, but he omitted to mention what the stakes were. I would like, if he would allow me, to substitute Mr. Bourne for myself in the operation of re-wiring, because he has had so much more practice, and can certainly wire with infinitely greater skill than I could. If, therefore, you will allow

him to be the jockey in the race, I venture to think that if the stakes are worth consideration Mr. Bourne will be quite willing to enter in the running.

Professor  
Ayrton.

As to the number of volts that are measured, this instrument measures a maximum of 90, and not 170, and therefore the remarks as to the amount of heat would not exactly apply.

Mr. W. B. ESSON: The instrument for a larger number is on the table.

Professor AYRTON [*after examining the instrument*]: I should not like to say that I was responsible for that one. Although it is on our principle, and has been constructed by our manufacturers, I have only seen it in this room, and have had no opportunity of testing it. Hence I must not be regarded as being in any way responsible for its action. But this voltmeter in my hand was made by Mr. Bourne under my eyes, and for the working of this one we are responsible; and the heating of this one is not excessive.

As to the answer to the question whether there is a measurable amount of self-induction in our new voltmeter, that depends upon whether you ask Professor Hughes or me. Professor Hughes would say "Yes," because he can measure it; I should say "No," because, although I have tried to do so, I have not been successful on account of its very small value. Practically, at any rate, we may say that the coefficient of self-induction of our voltmeter is extremely small, and that it introduces no error when the voltmeter is used to measure an alternating potential difference.

Next Mr. Swinburne entered into the question as to what exactly our voltmeter measured. Speaking off-hand, I should say that it measured the mean square of the potential difference, and that if the alternation of the potential difference followed a simple harmonic function of the time the mean potential difference was nine-tenths of the square root of the mean square of the potential difference as measured by our voltmeter. Slight errors may be introduced for the reasons Mr. Swinburne mentioned, but I think that all such errors will, when the wire is very fine, be extremely small. At any rate, one may safely say that whatever is the exact



Professor  
Ayrton.

function of the mean square of an alternating potential difference that is measured by a Cardew voltmeter, the same function is measured by our voltmeter.

With reference to the fuse, the wire is not, as Capt. Sankey said,  $\cdot 0004$  mill. in thickness. We have used  $0\cdot 0014$ , and now, in some of the instruments, we use  $0\cdot 002$ . I think Capt. Cardew generally uses  $\cdot 0025$ . Some of our voltmeters on the table are wound with  $\cdot 0014$ , and some with  $\cdot 002$  mill., but none with wire so fine as  $\cdot 0004$ ; so there is no difficulty about the fuse. I forgot to mention that there is an ingenious device with reference to the fuse. It was introduced by Mr. Butcher, the manager of the works of Messrs. Clark, Muirhead, & Co. Here is a rotating plug which contains six fuses, every one of which is of identically the same resistance, and any one of which can be *instantaneously* brought into action by turning this handle. Hence, if by any chance while using the instrument a fuse goes, it can immediately be replaced by another by simply turning this ebonite handle. Hence there is no necessity for opening the instrument, or for any delay to occur in the use of the voltmeter. Further, each fuse is adjusted so that the sensibility of the instrument remains the same when a change is made. Of course the sensibility of the voltmeter depends on the whole resistance in the circuit, so that if a fuse goes while using the instrument a great saving of time is effected by being thus able to substitute another fuse of exactly the same resistance. Hence not merely can a broken fuse be instantaneously replaced by another one without opening the voltmeter, but the sensibility of the voltmeter is not altered by the change in the fuse.

The PRESIDENT: Would you recommend the use of that dynamometer on board an electric launch?

Professor W. E. AYRTON: Certainly.

The PRESIDENT: In any kind of weather?

Professor W. E. AYRTON: Any kind of weather suitable for yachting is suitable for the use of our new voltmeter.

With reference to the question as to the slow charge of zero in the Cardew voltmeter, I believe that Captain Cardew, like many others, has suffered a great deal from want of knowledge on the



part of instrument makers; and I do know that many of the instruments made on his principle have not done him any particular amount of credit. Of course, if you take a platinum-silver wire, put it in an instrument without doing anything to it, strain it up, and then calibrate it, you will find after a short time, on recalibrating it, that you get a different sensibility. But the folly is to put an unstretched wire into an instrument. We always stretch a wire for a long period, and apply continual heatings and coolings before we calibrate at all. A paper was read by Mr. Bottomley at the British Association last year showing the effects that take place when a wire is subjected to continued heatings and coolings. After a time the wire acquires a permanent set, but at first it stretches a good deal. This goes on for some time, during which you must not calibrate your voltmeter; but when the wire has taken a definite set we begin to calibrate it; and the set having been fully arrived at, I venture to think there will be no further change. In some cases we have, by means of a little motor that started and stopped a current passing through a platinum-silver wire under tension, heated and cooled the wire 3,000 times before attempting to calibrate the voltmeter. I may mention in connection with this that a number of experiments are being carried out by Mr. Kilgour at the Central Institution on the coefficient of expansion of platinum-silver under considerable tension; and it is for that reason that we have not yet introduced the compensation into our voltmeters, which, however, is an easy matter to do. In order to introduce the compensation with certainty you require to know this fact—which I do not know yet with sufficient certainty—viz., what is exactly the coefficient of expansion of platinum-silver under a tension which is a considerable fraction of the breaking load; but when we know this there will not be the slightest difficulty in compensating this instrument. Take the double bicycle-wheel instrument: all you have to do is to make the rim of the wheel of a compound metal which has a coefficient of expansion equal to that of platinum-silver, and practically exact compensation will be attained. So far from the question of temperature having escaped our notice, weeks of experiments have been made with these instruments, which have been warmed up

Professor  
Ayrton.

Professor  
Ayrton.

for ten hours at a time to see what is the exact change of zero and exactly what is the change of sensibility.

I would draw attention to a remark I made at the last meeting. If anybody will look at this double bicycle-wheel volt-meter after the meeting, he will see that, whether it be vertical or horizontal, there is hardly any perceptible change of the zero. [*Exhibits instrument, and changes its position.*] This result has been obtained by balancing the pointer, by having the ivory hubs extremely light, and by having the pointer itself fairly light. The sharpness of the action on applying a varying potential difference is also wonderfully marked. We have also made the pointer more visible than it was the other day. A good deal has been said about pointers not being visible, but a watch is a good instrument in spite of the fact that you cannot see the hand 40 yards away. But by using excessively thin aluminium foil bent into a tube we have obtained pointers of great rigidity, which hardly bend when suddenly deflected; and the arrow-head, also made of thin aluminium foil, placed at the end of the pointer, enables it to be seen at a considerable distance. And the moment of inertia is so small that, as you observe, the pointer comes quite quickly to rest on the volts being applied to the instrument. I now take the current off, and the pointer deflects almost instantaneously to the zero.

As to the spring, of course the accuracy of this instrument depends upon the constancy of the magnifying spring. Well, now, just as the wire has to be subjected to a series of expansions by a current being passed through while the wire is under tension, so with the material of which the spring is made. We have known for years, and indeed pointed out in our paper before the Royal Society some years ago, that the spring should have a permanent set given to it, in accordance with the theory worked out by Professor James Thomson many years before that. When such a set is given to a spring, corresponding with the killing of the iron wire for an ordinary overhead telegraph wire, the spring shows no appreciable trace of a sub-permanent set if not strained too much. Further, the spring has but a small temperature error. As mentioned in the paper, five hours'

steadily heating of an ordinary magnifying spring ammeter up to  $40^{\circ}$  C. failed to show any variation of the zero from a temperature variation of the elasticity of the spring. In fact, the temperature variation of the elasticity was less than could be detected by the eye. Professor  
Ayrton.

The table given at the end of our paper, containing the results of tests extending over two years made on one of our magnifying spring ammeters, shows that the *age error* is also negligible. Hence I think that we are justified in concluding that not merely does the magnifying spring furnish an extremely convenient, frictionless, and comparatively massless multiplying gearing, but that it is comparatively unaffected by time or by variations of temperature.

The device mentioned by Mr. Gordon of turning the dial is also, you observe, employed in these instruments.

As is mentioned in the mathematics of the paper, the watts expended in heating a wire to a given temperature depend almost entirely on its length and temperature, and not on its thickness. Mr. Evershed's results are therefore borne out, and we see that very *great* economy in power wasted is obtained by using a short, very fine wire, instead of a longer, thicker one, in the construction of a voltmeter for a given range in volts.

I am afraid that at the last meeting an impression may have been formed that this large double-barrelled instrument was brought to disparage, or to show an antiquated form of, the Cardew voltmeter; but I must say that nothing was further from my mind. I was under the impression that the specimen of the Cardew voltmeter which we exhibited was one of the very best constructed, because I had bought it as such; it took a long time to make, and the excuse for the delay was, "We want to give Professor Ayrton a most perfect instrument." And not merely did the makers have *carte blanche* as to the time employed in its manufacture, but they had also *carte blanche* as to price; so that if the specimen of the Cardew voltmeter we exhibited was not one of the most perfect forms it was certainly the makers' fault, and not ours.

In conclusion, I beg to thank the meeting, on behalf of

Professor Perry and myself, for their kind reception of our portable alternating-current voltmeter, and for the warm approval that they have expressed regarding it.

A hearty vote of thanks was accorded to Dr. Fleming and Mr. Gimmingham for their paper, as also to Professors Ayrton and Perry for their paper, and to those gentlemen who had added to the interest of the discussion by exhibiting apparatus bearing on the subject.

The meeting adjourned until 8th December, 1887.

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The Sixteenth Annual General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 8th, 1887 Sir CHARLES T. BRIGHT, President, in the Chair.

The minutes of the Extraordinary General Meeting of December 1st were read and approved.

The PRESIDENT announced that the ballot-box for the deposit of voting papers for the election of President, Members of Council, and Officers for the ensuing year would remain open until 8.30 p.m.

Mr. J. Aylmer, Mr. G. Driver, Mr. J. Hookey, and Mr. R. Von Fischer Treuenfeld were appointed Scrutineers.

The names of new candidates were announced and ordered to be suspended.

The SECRETARY then read the following Report of the Council:—

### REPORT OF THE COUNCIL.

The Council have to report that the number of new members elected into the Society during the present year exceeds by 5 the number elected in 1886, and includes 5 Foreign Members, 12 Members, 66 Associates, and 36 Students, making a total of 119; while 17 candidates have been approved for ballot at the first meeting next month.

14 Associates have been transferred to the class of Members, and 13 Students to the class of Associates.

Our losses, on the other hand, have been heavy, those occasioned by death having unhappily been unusually numerous. We have thus been deprived of 3 Foreign Members—Messrs. E. Blavier, F. C. Guilleaume, and George West;—10 Members, among whom are Rear-Admiral Arthur, C.B., for many years at the head of the Torpedo School on H.M.S.

"Vernon," at Portsmouth; Colonel Sir Francis Bilton, Vice-President and Honorary Secretary, one of the founders of the Society, and to whom much of its early success is due; Colonel Sir John Bateman-Champain, K.C.M.G., R.E., our much-esteemed Past-President; Mr. S. W. McGowan, Chief Inspector of the Postal and Telegraph Service of Victoria, in which colony he represented the Society as its Local Honorary Secretary; and Major-General Hyde, of the Royal Engineers;—10 Associates, including that distinguished officer Colonel Sir W. Owen Lanyon, K.C.M.G., C.B.; Lieutenants R. B. Fulford, R.N., and E. C. Tyrell, R.N.; and Mr. Alfred Frost, the Society's able Librarian.

7 Foreign Members, 5 Members, and 14 Associates have retired from the Society during the year.

The Society continues deeply indebted to the President and Council of the Institution of Civil Engineers for their liberality in permitting its general meetings to be still held in their lecture hall, and in again consenting to our holding, when occasion arises, additional or extraordinary general meetings on some of the Thursday evenings intervening with those on which our ordinary meetings are arranged to take place—a privilege of which the Council hope to be able to avail themselves during the approaching year.

The papers read during the session will, in the opinion of your Council, compare favourably in point of interest with those of former years, and have embraced subjects on various branches of applied electrical science, as will be seen by the subjoined list:—

LIST OF PAPERS READ BEFORE THE SOCIETY DURING THE  
YEAR 1887.

DATE.	TITLE.	AUTHOR.
Jan. 27.—Telephonic Investigations ... ..		Prof. S. P. THOMPSON, D.Sc., Member.
Mar. 10.—On Reversible Lead Batteries and their Use for Electric Lighting ... ..		DESMOND G. FINE-GERALD, Member.
„ 24.—The Resistance of Faults in Submarine Cables ... ..		A. E. KENNELLY, Associate.
April 28.—Modes of Measuring the Coefficients of Self and Mutual Induction ... ..		Profs. W. E. AYERSON, F.R.S., and JOHN FRANK, F.R.S., Members.

DATE.	TITLE.	AUTHOR.
May 12.—	The Measurement of Self-Induction, Mutual Induction, and Capacity ...	W. E. SUMPNER, D.Sc., Associate.
„ 26.—	Underground Telegraphs ... ..	C. T. FLEETWOOD, Member.
„ 26.—	The Driving of Dynamos with Very Short Belts ... ..	PROFS. W. E. AYRTON, F.R.S., and JOHN PERRY, F.R.S., Members.
Nov. 10.—	Deep-Sea Sounding in connection with Submarine Telegraphy ... ..	E. STALLIBRASS, F.R.G.S., Member.
„ 24.—	On some Instruments for the Measure- ment of Electro-motive Force and Electrical Power ... ..	Dr J. A. FLEMING, Member, and C. H. GIMINGHAM.
„ 24.—	Portable Voltmeters for Measuring Alternating Potential Differences ..	PROFS. W. E. AYRTON, F.R.S., V.P., and JOHN PERRY, F.R.S., Member.
Dec. 8.—	On Safety Fuses for Electric Light Circuits, and on the Behaviour of various Metals usually employed in their Construction .. ...	ARTHUR C. COCKBURN, Associate.

The Council have awarded the Society's annual premiums in respect of papers read during the twelve months ending the 31st of May last as follows:—

*The Society's Premium*, value £10, to A. E. Kennelly, Associate, for his paper, "On the Resistance of Faults in Submarine Cables."

*The Paris Electrical Exhibition Premium*, value £5, to W. E. Sumpner, Associate, for his paper, "On the Measurement of Self-Induction, Mutual Induction, and Capacity."

*The Fahie Premium*, value £5, to Charles Thomas Fleetwood, Member, for his paper, "On Underground Telegraphs."

The Committee on Electrical Nomenclature and Notation have been assiduously summarising the various terms in general use in

Electrostatics,  
Electro-Kinetics,  
Electro-Magnetism,  
and  
Electrotechnics,



and have made considerable progress towards building together a chemical system, but some time must yet elapse before their Report can be submitted.

The Committee appointed to consider the question of the establishment of a *Consulting Laboratory for Electrical Apparatus* have given due attention to this important subject, and have made certain recommendations, which, as they involved the raising of a considerable sum of money, it was not thought desirable by your Council to attempt at the present moment to carry out.

The Committee on Rules and Regulations for the Prevention of Fire Risks arising from Electric Lighting, having been re-appointed by the Council, have nearly completed their revision of the rules and regulations which were issued by the Society in 1893.

Your Council have observed with much satisfaction the rapid increase in the number of young men entered as Students of the Society; and, in compliance with a desire expressed by that body, they made arrangements early in the year for "Students' Meetings" to be held in the Library of the Society on the Thursday evenings intermediate with those on which the general meetings take place, for the purpose of either reading and discussing papers written by some of themselves, or of discussing any of the papers that have been read before the Society. A Member of the Society—usually a Member of the Council—presides at each meeting, and the experience of the first year's working of the arrangement has proved most satisfactory, leading the Council to hope that the Students will, by reason of these meetings, gain sufficient confidence to enable them to bring forward papers and to take a greater part than they hitherto have in the discussions at the Ordinary General Meetings of the Society.

As a further encouragement to the Students to produce original communications, the Council have decided that such papers read at their meetings shall be eligible for competition for the Society's annual premiums, and for publication in the Society's Journal.

At the request of H.R.H. the Prince of Wales, as communicated by our Past-President Sir Frederick Abel, a subscription list was opened for contributions from the members of the Society towards the Imperial Institute to be established in commemoration of the Jubilee of Her Most Gracious Majesty the Queen.

The list of subscriptions has been published in the Society's Journal, with the exception of a few received since the issue of the last part; but, in consequence of so many members having already subscribed to the fund through other channels, the total amount is comparatively small, viz., £71 0s. 1d.

This being also the Jubilee Year of the Electric Telegraph in England, it was believed that the members of the Society would desire to mark the event by subscribing to a fund, to be called *The Electric Telegraph Jubilee Fund*, and which should be invested, the annual interest therefrom being devoted towards the improvement of the Society's Library. The amount subscribed up to the present time is £162 17s. 6d., and the Council hope that further subscriptions will be received from time to time, so as to render the fund more worthy of the occasion which it is intended to celebrate and of the purpose to which it is to be applied.

The Jubilee of the Electric Telegraph was further commemorated by a Dinner held at the Holborn Restaurant on the 27th July, at which the Postmaster-General presided, and in which the Society participated, the general committee by whom it was organised being composed almost entirely of members of the Society. Upwards of 240 gentlemen, all more or less directly connected with the past and present history of telegraphy, were present on the occasion; while the numerous congratulatory letters and telegrams received from the representatives of telegraphy in our colonies and other parts of the world (in nearly all cases also members of the Society) bore ample testimony to the extent to which the interest of the occasion was shared by them.

As will have been observed by the balloting list, the Council have not deemed it necessary to propose any candidate for election

as Honorary Secretary in the place of the late Sir Francis Bolton.

Considering it advisable to concentrate as much as possible the executive responsibility, and the office of Librarian having become vacant by the decease of Mr. Frost, the Council have placed the Library under the charge of the Secretary, who in all matters relating thereto will be in communication with the Library Committee.

The financial position of the Society continues to be satisfactory. A further amount of £92 has been received during the year on account of life compositions, making, with the balance of that account remaining uninvested at the end of last year, the sum of £114, which will be at once invested in the names of the Society's Trustees—viz., Sir Frederick Abel, Past-President; Latimer Clark, Esq., Past-President; and Edward Graves, Esq., Vice-President and Honorary Treasurer. The other funded property of the Society has already been vested in these gentlemen.

Your Council are of opinion that the title of the Society, even as amended some few years ago, does not sufficiently indicate the full scope of the objects which should be, and which are, its aim; and they believe that "The Institution of Electrical Engineers" would be a much fitter title, as embracing more clearly all those branches of applied electrical science which of late years have assumed such prominence and are becoming every day more important.

They purpose, therefore, to take the necessary steps for ascertaining the views of all classes of members upon the subject, previous to submitting the proposed change in a formal manner to a Special General Meeting of members some time during the coming year.

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\* Sir Wilham Thomson, Past-President, while expressing his willingness to act as one of the Trustees, strongly urged the desirability of some other Past-President, residing in or near London, being appointed in his place.

## THE LIBRARY.

## REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year—particulars of which are published from time to time in the Journal of the Society—amount to 71, including 60 presentations, which is considerably less than those received last year; but this circumstance cannot be attributed to any reluctance on the part of authors who are members of the Society to contribute copies of their works, for I am glad to report that my applications have been responded to most kindly. The number of works purchased is also smaller; and it would seem, therefore, that electrical literature has not, except in the case of technical periodicals, been of late so prolific.

The Society continues to receive, through the liberality of H.M. Commissioners of Patents, the specifications of all electrical patents. Of the total number of patents applied for during the 11 months ending the 30th November—viz., 16,359—the number having relation to electrical inventions was 920, or 5·62 per cent.

The number of visitors to the Society's Library during the year has been 373, of whom 53 were non-members.

Upon the recommendation of the Finance Committee, and with the approval of the Library Committee, considerable economy is being effected in the expenditure on binding, especially as regards periodicals.

A list of the Proceedings of other Societies, and of the technical journals received by the Society, is appended.

F. H. WEBB,

7th December, 1887.

*Secretary.*

## TRANSACTIONS, PROCEEDINGS, Etc, RECEIVED BY THE SOCIETY

## ENGLISH.

- Asiatic Society of Bengal, Journal and Proceedings.
- Cambridge Philosophical Society, Proceedings.
- Camera Club, Proceedings.
- Greenwich Magnetical and Meteorological Observations.
- Incorporated Law Society, Calendar.
- Institute of Patent Agents, Transactions.
- Institution of Civil Engineers, Proceedings.

Institution of Mechanical Engineers, Proceedings  
 Iron and Steel Institute, Proceedings,  
 Liverpool Engineering Society, Proceedings.  
 Physical Society, Proceedings.  
 Royal Dublin Society, Transactions and Proceedings  
 Royal Engineers' Institute, Proceedings.  
 Royal Institution, Proceedings.  
 Royal Meteorological Society, Proceedings  
 Royal United Service Institution, Proceedings.  
 Society of Arts, Journal.  
 Society of Chemical Industry, Journal  
 Society of Engineers, Proceedings,  
 University College Calendar.

#### AMERICAN.

American Academy of Science and Arts, Proceedings  
 Canadian Institution of Civil Engineers, Proceedings,  
 Franklin Institute, Journal of  
 John Hopkins University Circulars.  
 Library Bulletin of Cornell University.  
 Ordnance Department of the United States, Notes  
 Smithsonian Institution Reports.

#### FRANCOIS.

Société Belge d'Electriciens, Bulletin de la.  
 Société Française de Physique, Séances de la.  
 Société des Ingénieurs Civils, Memoires.  
 Société Internationale des Electriciens, Bulletin de la  
 Société Scientifique Industrielle de Marseille, Bulletin de la.

#### LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

#### ENGLISH.

Electrical Engineer.  
 Electrical Plant.  
 Electrician.  
 Engineer.  
 Engineering.  
 English Mechanic and World of Science.  
 Illustrated Journal of Patented Inventions.  
 Indian Engineer.  
 Industries.  
 Invention.  
 Machinery Market.  
 Mechanical Progress.  
 Military Telegraph Bulletin.  
 Nature.  
 Patent Office, Official Journal of.  
 Philosophical Magazine  
 Scientific News.  
 Telegraphic Journal and Electrical Review.

**AMERICAN.**

Electrical Review.  
Electrician and Electrical Engineer.  
Journal of the Telegraph.  
Science.  
Scientific American.  
United States Patent Office, Official Gazette of.

**FRENCH.**

Annales Télégraphiques.  
Cosmos les Mondes.  
L'Electricité.  
Journal de Physique  
Journal Télégraphique.  
La Lumière Electrique.  
L'Electricien.  
Revue Internationale de l'Electricité et de ses Applications.

**GERMAN.**

Annalen der Physik und Chemie.  
Beiblätter zu den Annalen der Physik und Chemie.  
Centralblatt für Elektrotechnik.  
Electrotechnischer Anzeiger.  
Elektrotechnische Zeitschrift.  
Verhandlungen des Vereins zur Beförderung des Gewerbflusses.  
Zeitschrift für Elektrotechnik.  
Zeitschrift für Instrumentkunde

**ITALIAN**

Giornale del Genio Civile.  
Il Telegrafista.

The PRESIDENT moved—"That the Report of the Council, "as just read, be received and adopted, and that it be printed "in the Proceedings of the Society."

Mr. J. E. H. GORDON seconded the motion, which, being put from the Chair, was carried unanimously.

Mr. A. STROH: I have great pleasure in moving—"That the "cordial thanks of the Society be presented to the President, "Council, and Members of the Institution of Civil Engineers for "their liberality in continuing to allow the Society to hold "its fortnightly meetings in the theatre of the Institution, "and for so kindly again extending the privilege for a certain "number of additional meetings being held during the session."

Professor D. E. HUGHES seconded the motion.

The PRESIDENT: There can be no difference of opinion upon the

motion before you, as the least we can do in the way of acknowledgment for our hospitable reception here, by what I may call our mother Society, is to pass such a resolution unanimously.

The motion was carried by acclamation.

Sir DAVID SALOMONS: I have a resolution to move which I am sure will be received quite as heartily as that which has just been adopted. It is often said that people who are absent are frequently forgotten altogether; at all events, there is an old German saying to that effect. But there is another saying—an English one—that “absence makes the heart grow fonder.” I am not sure whether that is the right way of putting it, but my resolution is—“That the thanks of the Society be presented to those members who “so ably represent it abroad as Local Honorary Secretaries and Treasurers, for their continued attention to its interests.” I may tell you that the business of the Local Honorary Secretaries abroad is certainly very onerous. It is not the same here, where the members are more or less concentrated, where a directory can be taken out and particulars found of the whereabouts of persons. But abroad search has to be made all over the place to find what gentlemen are likely to join us, and likely to be useful when they have joined. I learn from our Secretary that the two Local Honorary Secretaries last appointed, viz., Mr. Kingsford in Peru, and Mr. T. R. James in Victoria, have done exceptionally well in already bringing a great many new members to our ranks, and I move the resolution with very great pleasure.

The PRESIDENT seconded the motion, which was heartily carried.

The ballot-boxes were withdrawn.

Mr. C. E. SPAGNOLETTI (Past-President): I have very great pleasure indeed in moving—“That the thanks of the Society “are due to Mr. Edward Graves, V.P., Honorary Treasurer, for his “continued watchfulness over the financial interests of the Society.” We all know how carefully Mr. Graves looks after our interests, and we also know how difficult it is to get money from him unless a very good reason for it is forthcoming. He has kindly acted as our Honorary Treasurer for many years, and will continue to do so, I hope, for many years to come.



Mr. E. B. BRIGHT seconded the motion, which was carried unanimously.

Mr. EDWARD GRAVES, V.P.: I thank you very much for the compliment you have paid me, though I fear I but little deserve the honour. I am glad to say that the finances of the Society are in a much healthier condition than they were some years ago.

Mr. EDWARD GRAVES, V.P.: I beg to move—"That the thanks of the Society be presented to Mr. J. Wagstaff Blundell and to Mr. Frederick C. Danvers for their kind services as Honorary Auditors." The Auditors have a somewhat thankless task. They have much work, they pursue it in the dark, and little is known of it; but on the careful discharge of those duties the successful result of our financial balance-sheet must greatly depend.

Professor G. FORBES seconded the proposition, which was carried unanimously.

Mr. ALEXANDER SIEMENS: We have another debt to pay for services rendered voluntarily, and I beg to move—"That our best thanks are due to our Honorary Solicitors, Messrs. Wilson, Bristows, & Carpinael, for the kind and valuable services rendered to the Society by them through G. L. Bristow, Esq." Those legal services keep us out of harm's way, and have a deal to do with the smoothness in which the working of the Society has been conducted.

Mr. W. H. PREECE seconded the motion.

The PRESIDENT: It may be supposed that the Society does not very often get into law, and, for that matter, I do not remember any law proceedings in which the Society has been engaged—that is, in litigation; but still there are matters, such as the drawing up of agreements, the settling of rules and regulations, and so forth, in which we have had most valuable assistance from our Honorary Solicitors.

The motion was carried unanimously.

Mr. G. L. BRISTOW: I am sure I thank you very sincerely for your kind vote of thanks, and also for the manner in which it has been proposed and seconded, and I am indebted for the kind support of your President. I am afraid that I am before you to-night almost under false pretences. Others before me have

received your thanks for various good services rendered during the past year. I am in the position of having done little or nothing at all; but still, if there had been occasion for any legal assistance, I am sure I should have been very pleased to have rendered it; and in the future, should there be occasion, I hope I shall not be backward in doing my best. I again beg to thank you.

The PRESIDENT: While the Scrutineers are looking over the ballot papers, we will proceed with the ordinary business of the meeting.

The following paper was then read:—

ON SAFETY FUSES FOR ELECTRIC LIGHT CIRCUITS,  
AND ON THE BEHAVIOUR OF THE VARIOUS  
METALS USUALLY EMPLOYED IN THEIR CON-  
STRUCTION.

By ARTHUR C. COCKBURN, F.C.S., Associate.

I venture this evening to bring before your notice a subject which, considering the rapid development of electric lighting, I think is of importance, namely, electric fuses, or cut-outs, for assisting in the protection of buildings from fire arising out of the employment of electricity for light or transmission of power. With the use of electricity spreading as it is so rapidly in all directions, it behoves every electrical engineer to see that no discredit be thrown upon the use of electricity as a domestic agent through bad work and subsequent chance of fire breaking out, and to prove to the public at large that an installation of the electric light, or of power, is not a source of danger, but that it is, when carefully put up and looked after, one of, if not *the* safest means of providing both light and power. We have had recently the burning of the Exeter Theatre, the Opera Comique in Paris, and other buildings in which gas was used, and which, from the very nature of the proceedings carried on in them, were rendered very liable to fire—examples of the need of a safer method of lighting. Of course the first outcry was that electric light should be employed as one of the safeguards, and quite rightly

so; but at the very time appears a letter to the effect that the employment of electric lighting was itself a source of much danger through the overheating of the cables, &c. Now, on those who do not know better, how does this statement act? Why, they jump to the immediate conclusion that the electric light is something to be greatly feared and avoided; and it would take a great deal to convince them to the contrary. Now we, as electricians, must prove to the public that if the work is undertaken by reliable firms, and erected by competent hands,—if the cables are of the right size and properly insulated, the switches of the right sort, and the leads properly protected with *good* and *efficient* fuses,—such overheating cannot take place. By doing this we shall greatly advance electric lighting in the minds of the public as the safest and best of all artificial methods of illumination, and tend to make its early adoption general. Now I have used the words *good* and *efficient* fuses, for unfortunately the rage after everything cheap, no matter how bad or inefficient, and the keen competition to get work at any price, has led to the employment of some fuses which are anything but what they should be. Among the many statements that have been made with regard to electric lighting, we all remember how it was said that a cut-out was an absolute protection against an electrical fire, and how this was believed in by a large majority of the public until Mr. Musgrave Heaphy's letter appeared in the *Times* in 1883 describing a fire into the cause of which he had to investigate. In this case he found the conductors most carefully put in, and well protected by delicate fuses, yet an electrical fire broke out. No cut-out, no matter how good, will be of any use in the case of a breaking of the lead wire and consequent arcing, or the overheating of a switch, for example. This was followed by a statement that bad cut-outs were a source of danger. It is unfortunately true that many of the fusible cut-outs that have been in the market are not only inefficient as protectors against fire, but have been themselves frequent causes of that which they professed to prevent. We have now many cases in which cut-outs, failing to act when required, have themselves become the *source of a fire*.

I would here mention that my attention was first called to the investigation of this subject by a visit to Mr. Musgrave Heaphy, whose great experience of electrical fire risks we all know. In the course of an interview with him I gleaned much information, and he kindly gave me permission to examine his collection, only reserving those samples which he could not honourably show me without permission of certain firms. This led me to see the want of an absolutely reliable cut-out. An inefficient cut-out, without any fire-proof protection round it, placed between floors and ceilings, under roofs, behind skirting boards, and other similar spots where, if a fire were to break out, it would at once get a good hold, adds to the risk. For example, you will find cut-outs of wood that has not been rendered fire-proof, with a plain piece of wire resting on its dry surface, ready, directly the wire becomes *red-hot* (and not broken, which is not an unfrequent occurrence), to take fire and communicate it to the building. Again, I have found a short piece of grooved wood, with two plates of brass inserted, the extreme width of break between them being only three-eighths of an inch, with a short piece of fuse wire fixed across them, and resting on the wood, which was all charred below. I maintain that such work as this is not only bad, but is the very thing to bring electric light into disrepute. It is against these false protectors that I ask all electrical engineers who have the welfare of their profession at heart to set their faces. True, in the case of a *dead* short-circuit such a fuse may act; but, as I propose to demonstrate to you, such wires so fitted are dangerous, and for currents in slight excess above the normal will often actually become *red-hot* long before giving way. A fuse wire should never be less than  $1\frac{1}{4}$  in. long, even when fitted into the best fuse-holder. Now, above all things, it is necessary that electric light installations should be so erected as to ensure public confidence in their freedom from danger. All the arrangements should be of the best kind. It must be made literally impossible for any wire, whether main or branch, to become overheated by the passage of an excess of current through it. That is to say, every wire must be protected by reliable cut-outs that will instantly break

the circuit if the current attains a strength in excess of that which the wire is able to carry with safety; and the fuse should never allow a current of more than 100 per cent. above the normal to pass; and I would prefer even a less margin than this. It is also of importance that the lamps should be properly protected, and this can easily be done when one employs a cut-out which is so constructed as to act with precision and promptitude when a definite strength of current is reached; less margin being allowed for excess than above mentioned. True, one does not like to be put in darkness, through the fuse being too sensitive and acting too easily; but sufficient allowance can be made for a slight and harmless excess of current, and a little above this point the lamps would become their own fuses, so even then darkness would result. This objection, therefore, falls to the ground. Moreover, since the cost of one cut-out is less than the fortieth part of the cost of one lamp, the cost of protecting even single lamps is worth incurring. In cases where accumulators are used, they, too, need protection for themselves—not always to be obtained by the use of a magnetic cut-out merely, which may or may not act, and which, when not acting, is worse than useless. Moreover, when accumulators are used in an installation, especial care should be taken that the fuses are efficient; because the cells are capable of damaging themselves, as well as the wires, by reason of the immense currents which they instantly furnish when short-circuited. In this case the protection of one of the two wires is not sufficient. A double-pole cut-out should always be used, both on the mains themselves and on all principal branches. For example, a single-pole cut-out is placed, say, on the negative lead. Now by some accident this lead comes into contact with a gas pipe, or gets earthed somewhere between the fuse and the dynamo. Now of what use will this fuse be if a short-circuit takes place between the positive lead and, say, the gas pipe? The current would pass by way of the positive lead and the gas pipe, and not through the fuse at all. So if an electrical fire occurred it would burn itself out, the fuse not acting. If, however, the positive lead were also protected with a fuse, then directly a short-circuit occurred one or the other fuse

would act and the circuit be broken. The rules for fitting up electric light installations show how needful cut-outs on both leads are considered by those of experience.

Then, again, is the armature of the dynamo to be made its own fuse in the event of a complete short-circuit of the mains taking place?

Another very important reason for having good fuses is the protection from unseen leakage to earth, or from lead to lead. If a fuse be well chosen, a moderate leakage can be detected by the fuse acting through the passage of the excess current over that required for the lamps themselves.

Now what are the essential points in a good fuse or cut-out?

First, and foremost, the fuse itself must have a definite breaking or acting point, so that the user can be certain of the fuse acting immediately a definite percentage of excess of current is reached. Thus, for example, say the normal current in a circuit is to be 5 ampères: if one allows, according to the rules in practice, 50 to 100 per cent. margin for excess, that would mean that a fuse which would act with certainty when 10 ampères passed through it would have to be inserted. A fuse marked to go off at 3 ampères and not acting till 9, or one marked to go at 5 ampères not acting till 26 ampères pass through it (samples of which I have had), are clearly inadmissible. Take the latter one as a case in point. Marked to go at 5 ampères, the chances are it would be used to protect a 3-ampère circuit; yet we see that it would not act till the current in the circuit increased to something like 800 per cent. of its normal value. The fuse itself should act with certainty within 5 per cent. to 10 per cent. of the current to which it is issued to fuse at, and in this way a definite total excess can be allowed and determined, and the circuit therefore protected. Fuses should not easily alter their breaking point, or much change with time. Ordinary fuse wire has this disadvantage, as will be presently shown. One of the chief objections to using cut-outs in which moving parts are required is their possibility of sticking at the critical moment. Another important point is the length of the fuse. Length plays an important part in the question of definite



breaking point. A reference to the curves on the wall will clearly show this; but we shall deal with the matter more in detail further on, so will not stop to discuss it now. The material of the fuse should be such that it will not oxidise readily. If a wire or foil be used, it should hang free, and not lie upon the holder. For if a wire or foil touches the case anywhere else than at its ends, there is greater tendency for heat to be conducted away from the wire or strip, and at the same time unnecessary heating of the holder results. This fault may cause the holder to crack, as in the case of vitrite—of which there is a sample upon the table—or, as in some cases where bad or unprepared wood is used, the red-hot wire may set fire to the wood before it gives way itself.

The fuse-holder or cut-out case should be made so that only the proper size of fuse can be inserted in it. For example, say a holder is issued with contacts capable of safely carrying 8 ampères, and is sold as such, then the fuse which would not act till, say, 12 ampères were passed through it should be of such a size as not to fit into such a fuse-holder. Unless the metal contacts of the fuse-boxes are made unnecessarily thick, this is important, or the heating of the contacts themselves may result. There is a still more important reason why only a proper size of fuse should be used with certain size contacts—viz., that the exact breaking point of a fuse depends very much upon the size of the metal contacts with which it is used. On the curve for tin No. 2 you will see the difference of fusing point for various lengths of similar wires of 37 mm. diameter. This difference is, as you will see, very large, and clearly shows that if accurate work has to be done the fuse must be marked with the current at which it will fuse when used in its own holder or box, and must not fit any other form of holder. In other words, it must be tested in combination with definite size contacts, and issued to be used with such. Care should be taken that the material of the fuse-boxes is good. No wood, except box-wood, should ever be employed unless specially made fire-proof. If the fuse to be used can get red-hot, porcelain, well baked and double-glazed, is to be preferred, or slate.

The metal contact-pieces and screws should be thick enough



to carry the current, and that there are the larger currents they are possible of having passed occasionally through them. These fuses, which are provided in most boxes for the holding of the lead wire, in addition of a strip of rubber wood, do no harm. I am glad to know that there are cases in practice where this is systematically carried out. I do not, however, recommend soldering; but beyond a doubt it adds to the perfection of the contact, and makes a good sound joint. In the case of sealing wires of large diameter, and stranded wire, I prefer that the ends of the wire or cable should pass clean into the said contact-pieces, and then be held with a screw, in preference to simply passing round or under a wire-head.

The cases enclosing the cut-outs should be of such sizes and shapes, and the arrangements of holes for insertion of the conducting wires should be such, that any workman could have no difficulty in fitting them up. Further, the incoming and outgoing wires should not have to cross or much alter their ordinary course. The diagrams show how this may be effected.

The fuse should be easy to replace when broken, and be so arranged that good contact is certain; otherwise heating at the surface of contact between the fuse and its clamps will result, and the safety of the fuse be thereby endangered.

Whenever possible, the fuse should be in sight; a glass cover to the box is preferable. In many cases this is important, as instances have been known in which, because a fuse acted too frequently to please a workman, a piece of copper wire has been placed behind the foil, and so the fuse rendered practically useless. This is most likely to be done in the larger size cut-outs, so here I particularly recommend glass or mica fronts. The transparent front is also useful in quickly detecting if the fuse has acted, without the delay of unscrewing or taking off a cap or lid to see which is the fuse that has to be replaced.

Now to sum up what we seem to want.

First of all, it is evident we want a good fuse-box or holder, the material of which is good; and if there be any chance of the fuse wire becoming sufficiently hot to scorch ordinary wood, slate or earthenware should be employed, or a wood box lined with *bestos*.

It is also requisite—That the metal contact-pieces, or clamps, be large enough for the current they are to carry.

That the arrangements for fixing the fuse-box be simple and intelligible.

That good connection be provided for the conducting wires.

That the arrangements are such that a broken fuse can be easily and quickly replaced, and good electrical connections made.

That the length of fuse shall not be less than  $1\frac{1}{2}$  in. in the clear; shall fit only its own box; and shall have a *definite breaking point*. That it shall not possibly be able to oxidise or become *red-hot*.

That the fuse-box should be well within sight, so that any tampering with box or fuse can be easily seen; and in the case of magnetic cut-outs in which mechanical motion is employed, that there should be good clean pivots or joints.

On the table before you are a large variety of fuses and cut-outs, all made more or less to fulfil these requirements.

One of the earliest forms is that in which a piece of wire, of lead, copper, or other metal or alloy, is used. Now there are many points in favour of wire as a fuse, not the least of which is the ease of use; but even this ease has its abuse.

I have seen a cut-out made of a block of wood, with two small plates fixed to its surface with screws; a length of fusible wire was wound several times back to and fro across the surface of the wood, the person who fixed the same expecting the small screw to properly hold all the rounded wires then placed under it. In this case heating was set up at the imperfect joint of the screw and wires, and fire was the result. It is so easy with a long length of wire to wind it on, disregarding the very object for which the cut-out is put up. If the wire had been in a definite length suitable to the holder this would not have been possible, save by deliberate action.

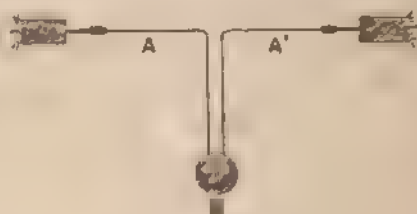
Another strong point against ordinary wire is that, within a large range, any size wire can be inserted in any size holder. Thus, in a box only fit for passing a current of, say, 5 ampères, a short, thick piece of wire capable of passing 60 or 80 ampères (and of no service whatever as a protector of the

leads) can be inserted; and, as the curves for tin No. 1 show, a slight alteration of length means, in the larger size wire, a great alteration in its fusing or breaking point. Take, for example, a tin wire 22 mils. diameter. You see that 5 inches fuse with a current of 5 ampères, 3 inches 5·4 ampères, and 1 inch 8 ampères. A wire of larger diameter will show this even better. Here is one of 62 mils. diameter. Now 5 inches of it fuse with a current of 19 ampères, 3 inches 23·5 ampères, 1½ inches 34 ampères, and 1 inch 44 ampères—a difference of 10 ampères for only half an inch difference in length. Now you will notice that, as the length of wire taken gets shorter, so the curve becomes steeper; and this is most noticed in the case of the larger fuse wires. A matter of a fraction of an inch makes all the difference in the actual fusing point. Here are curves for copper, iron, tin, tinfoil, lead, and other metals, all of which show similar effects; and I wish to thank Mr. Handcock, one of my co-assistants, for the great amount of time and thought he has spent in helping me to carry out the work of this investigation, and in plotting the various curves. I also have to thank my friend Mr. Thomas for the great assistance he rendered me in the earlier parts of the work. These curves are the results of many hundreds of experiments, and all the tests were made with the same pair of contacts (except, of course, No. 1 and the lower curve in No. 2, in which the question of effect of mass of contacts was the object), and under the same conditions with regard to instruments, position, &c.; also avoiding unequal errors from loss of heat by radiation, conduction, &c., as much as possible. To get at a fair average for the true relative breaking points, the time allowed for the experiments varied in each case—from a current suddenly put on, to a current steadily increasing in strength for a quarter of an hour. To more nearly approach actual practical work, in most cases, at the end of each set of experiments, when we were fairly sure of what current was required to cause the fuse to act, a current just below the critical strength was caused to pass through a fuse for some time, to allow the flow of heat from the wire to the contact-pieces, and their heating, to become uniform; the current was then gently increased till rupture of the wire was the result.

This dependence upon length is a strong point against ordinary fuse wire; but still stronger is the fact that if the current flowing through a piece of fusible alloy or tin wire be only slightly increased above its normal value, then, instead of the wire fusing, it heats up, and a film of oxide forms upon its surface. The curves for this are well shown by the dot and dash line on diagram for tin No. 3. At first this film is thin, and easily artificially broken; but a slight increase of the current causes this film to grow with great rapidity and form a hard shell, or outer skin, upon the wire. This skin is capable of holding up the molten metal inside in a manner similar to a pipe conveying water. The wire when in this state will allow a much stronger current than the normal to pass through it, without its giving way, than would have been sufficient to fuse it in the first instance. The resistance of the wire is now increased by reason of this partial conversion of its metal into oxide. Now resistance to the passage of an electric current means the development of heat at the place where the resistance is offered. A rise of temperature follows, and the current also being above the normal the wire soon becomes red-hot, and capable of setting fire to anything of a combustible nature it comes in contact with, in which case causing a fire immediately. There is still a further objection to the use of ordinary fuse wire, namely: If the wire when in a molten state, as just described, hangs so that one part is at a lower level than the rest, the molten metal will flow through the tube of oxide from the portion of the wire at the higher level to that at the lower. This was also noticed in the experiments conducted by Mr. Heaphy; and he advocated placing the fuse wire in a vertical position, so that the molten metal would run down the inside of the tube, as it were, to the bottom, and so assist rupture. But my experiments show that this cannot always be relied upon, though a step in the right direction; for while the fuse thickens at the lowest parts, the upper ends or parts thin gradually away, leaving only the oxide to hold it up. This causes the fuse to become very unreliable. Sometimes the thin film of oxide may give way with the passage of a current far below the normal; at others it may become red-hot and cause fire.

Now we come to strip foil. I cannot say that foil is reliable, but still it has a great advantage over plain wire. One objection to its use is its being so thin and having to be more or less attached to a supporting substance over the whole of its length. Foil also will oxidise, and does not go off when made up as a fuse at as definite a mark as is desirable. For instance, here are some fuses marked to go at 3 ampères, which went off at 9 ampères; one marked to go at 9 ampères went off at 20 ampères, &c. Here one of another maker, marked to go at 5, went off at 26.1 ampères, or at 400 per cent. increase of current. The above curve will show you the results of a number of experiments with foil that was not made up as a fuse with support for the tin. The usual length of the made-up fuse is between 1 and  $1\frac{1}{2}$  in. in the clear, or the bad part of the curve. Again, it is not so easy to be certain of the same breadth and thickness as one can be of the diameter of a carefully drawn wire.

In 1879 Prof. S. P. Thompson invented an improved form of fuse, or cut-out. It consisted of two wires, A, A' (see Fig.),



of iron, which were fastened together at B by means of a ball of lead and tin, or other easily fusible metal, which was cast on to them. On the passage of a current of sufficient strength through the fuse the wires and ball became heated, the lead melting, dropping down, and so allowing the wires to fly apart and break the circuit.

A somewhat similar form of fuse is that due to Sir William Thomson. It consists of two springs with their tips soldered with a fusible solder. I have not tried this fuse for definite breaking or fusing point. The springs themselves form the chief resistance to the flow of the current, and become heated, thereby melting

the solder and flying apart. Then we have electro-magnetic cut-outs, about which much that is good might be said when they are carefully made, and when they are wanted to protect main leads; but when it is required to protect the smaller branches (and the protection of the branches is very important, as a very moderate current of but a few ampères can do much damage if let loose) this form of cut-out then becomes both cumbersome and too expensive. But so long as they do not become stiff and are well looked after they are excellent for protecting the main leads from the dynamo. They also have the advantage of being easily replaced by pushing over the armature. I have, however, seen one of these instruments burnt up—I believe simply due to the moving parts becoming clogged or dirty.

The curves (Plate I.) show the results of numerous experiments tried with various metals. For instance, take lead. This metal melts at  $324^{\circ}$  C. Compared with silver as 100, its conductivity is about 7.7. It is very liable to oxidisation, and one of its greatest drawbacks is the difficulty of drawing it into a fine wire of uniform diameter, due to accidental stress and other causes. From the curve you will see that the breaking point of lead is very greatly dependent upon length; and our experiments prove this unreliability to be owing chiefly to oxidisation. Copper was next tried. This has a melting point of about  $770^{\circ}$  C., is a good conductor, but has the defect of becoming red-hot before it gives way; also, if it is heated up to a red heat, then cooled, then heated up again, it varies its breaking point and becomes very uncertain. Reference to the curve for copper will clearly show that length affects the definite breaking point very much, a difference of  $1\frac{1}{2}$  in. in a wire of 24 mils. diameter causing as much as 20 ampères difference in the wire's breaking point; it also oxidises, and for certainty of action cannot be recommended. Platinum was carefully tried. This has a very high melting point, and is a bad conductor of electricity and heat. From this latter cause it is not so subject to variation from difference of length, but it has a very serious objection owing to its high melting point; it becomes red, in fact white, hot long before its breaking point is reached. Reference to the curve for this metal will show that a



current of 14 ampères will make  $1\frac{1}{2}$  in. of platinum wire 30 mils. in diameter red-hot, but it takes a current of 24 amperes to fuse it; in other words, if a current of, say, 18 or 20 ampères pass through, the fuse would remain white-hot, resembling an incandescent lamp, and yet not act as a safety fuse. Platinum must therefore stand condemned as essentially unsafe. Independently of its want of safety, the expense when we come to the larger size fuses, and the cost and risk of keeping a stock of spare fuse wire, which must be on hand in case of a breakage, would be prohibitive.

The curves for iron follow the platinum curves very closely, save in the differences between the currents required to make the wires red-hot and those required to melt them; and the melting point is not so well-defined with iron as with platinum. A great and fatal fault with iron is its great liability to oxidisation both from current and damp; the latter alone quite prevents its use.

The importance of a definite length being employed for the fuse is clearly shown by most of the curves. In all the experiments of which the curves are the result the same pair of contacts were used, because a variation in the mass of the metal of the contacts would cause a variation in the breaking point, and then the result could not have been compared, as our curves for tin No. 2 will show. You will notice in these curves that the lowering of the breaking point is nearly uniform for all the lengths from  $\frac{3}{4}$  in. to 5 in., viz., about 9 ampères; the form of the two curves remaining otherwise the same.

Now how are we to obtain a definite fuse with these difficulties in our way?

In the early part of the present year I undertook a series of tests of different wires for a large electrical firm, with a view to forming some table of breaking or fusing points, &c. Some of the curves then obtained are before you. In the course of these experiments I particularly noted the oxidising and then heating to redness, before fracture, of most of the wires used. You will see on the curve for tin No. 1 (Plate I.) a *dotted line*. I found that the safe length of wire to use to prevent risk from fire varied with the sectional area of the wire under test, and must not be



less than the length shown on the left of the dotted line; shorter lengths than these are dangerous. For large wires one was pretty safe, but when I came to test the smaller gauge wires I found that as great a length as 3 to 4 in. was required to keep clear of danger—a length altogether impracticable for ordinary commercial lighting. This led me to more carefully examine the subject, and in doing so I noticed that in every case before the wire got red-hot it oxidised, as before explained. So stiff was the skin that it would stand a moderate push without giving way. The molten metal, being thus unable to escape, became hotter under the excess of current, and the whole mass became red-hot and glowing, and able to set fire to wood that had not been made fire-proof. Clearly the thing to accomplish was to break down, or assist to break down, the shell of oxide. Various plans suggested themselves, such as springs, &c.; but the most simple and effective, I found, was to weight the wire, by means of a piece of similar wire, or by a small weight slung upon it. This was tried with magical result. Not only was the wire absolutely prevented from becoming red-hot, but the film of oxide was also prevented from forming at all. Thus the wire is always kept bright, clean, and its sectional area at its ends undiminished by any thinning. Further experiments showed that by using suitable weights the question of length of wire became of less importance, and, moreover, that the wire had acquired a definite breaking point. Of the various metals and alloys tried I have found pure tin to answer best. This metal has a lower melting point than lead or zinc, does not readily oxidise, and in fact is, as you are aware, largely used to cover ironware goods and protect them from oxidation. Taking silver as 100, the conductivity of tin for electricity is 11·4, that of lead being 7·7.

At about twice the temperature of boiling water pure tin becomes plastic, or semi-molten, and melts at  $235^{\circ}$  C.; at this temperature it will not char wood. It is when in this state of paste, and before it has time to oxidise, that the action of the suspended weight is felt. The wire in this pasty condition, though able to hold itself up, cannot support the extra weight of the shot, so drops the weight at the first moment of softening,

and the connection is thus broken. As you will see by these samples on the table, the break is quite clean and certain. Another fact that helps to give the fuse such a definite breaking point is that the wire, being at such a low temperature when it acts, is not so affected by radiation as it would be if it were hotter before action took place; and greatly from this cause the fuse always acts at the same current, whether in the open or shut up in a confined space or box. You will now see that there is no absolute need to use any other than a wooden fuse-box, as the wire cannot possibly become hot enough to even scorch the wood, much less set fire to it; still I insist that all my fuse-boxes, if made of wood, are lined with asbestos, but I even prefer earthenware if possible.

I choose lead as my weight—

(1) Because of its high specific gravity, which allows the shot being kept small and not covering so much of my fuse wire.

(2) Because of its low specific heat, it does not tend to abstract so much heat from the wire and impede its quickness in acting.

From my experiments I find that the fuses so constructed can be certified to act within 10 per cent. of the current marked for them to fuse at, and in most cases within 5 per cent. or less. It is therefore clear that a fuse that cannot be a source of danger from becoming red-hot, and which also has a definite breaking or fusing point, can be made of cheap material and relied upon.

My fuse, loaded with its leaden ball, is shown in Fig. 3, Plate II.

In practice, I find it advisable to protect the ends of my fuse from damage, winding the wire round eyelets, which also serve to form convenient ends for making a good contact.

It is possible to make fuses to protect single lamps.

To make certainty even more sure, the fuses are all made to standard lengths and diameters, and as far as possible all fuses are tested before being sent out to nearly their breaking point. One of the boards for carrying out such tests is upon the table.

It may be thought that the small weight always hanging on the wire would affect it after a time. All my tests in this

*Ampères.*



*Ampères*





TYPE C.  
MAIN LEAD  
CUT OUT  
COMPOUND

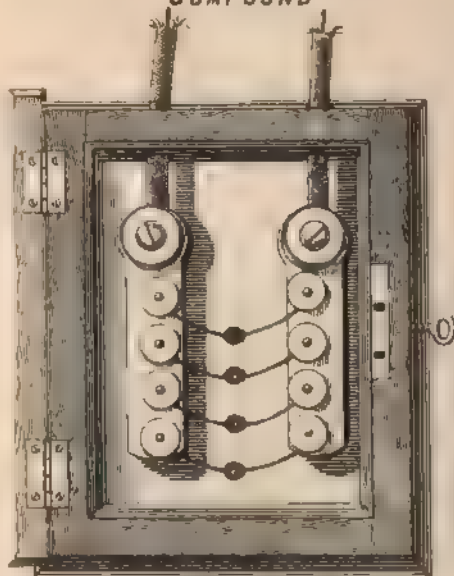


Fig. 9

WALL & CEILING  
CUT OUT



Fig. 10



direction prove that this is not so; and even though a current of a strength only just below the breaking current be sent through a fuse for months, the sectional diameter of the wire at its ends was undiminished, and there is no sign of oxidisation. This follows, because, if the wire had become sufficiently soft to thin, a properly proportioned weight would have caused the wire to rupture immediately, the action of the load or weight being felt at the first moment or sign of softening.

Reference to the various diagrams (Plate II.) will show how the boxes are used and fitted, also how the crossing of any of the local wires is avoided.

I wish to thank the Electrical Power Storage Company for their kindness in lending the accumulators from which current is being supplied; also to thank Messrs. Binswanger & Co., Messrs. Sudworth & Co., and Messrs. Woodhouse & Rawson, for samples of cut-outs, &c.; and Messrs. R. & J. Beck & Co. for their kind loan of microscopes.

I trust that I may fairly claim to have carried my investigations to a point beyond any reached by those who have preceded me in this direction.

The PRESIDENT: I shall be glad to hear the remarks of any gentleman interested in this subject. I would like to point out that the idea of a fusible wire, or piece of wire, for protecting apparatus belongs to the same family as many of the lightning protectors used for many years to avoid damage by abnormal currents in connection with telegraph instruments, used at the ends of submarine cables and land lines. The drawing before us of the form introduced by Prof. S. P. Thompson has been applied by me, with a very small difference of form, in 50 different cases in which a rod has been supported by the upper one of a number of platinum wires. Upon the fusing of the top wire the rod would drop down to the next below, and so on in succession until it finally dropped down to "earth" and so shut the circuit off altogether. A drawing and description of this apparatus is given in Ternant's work\* on the telegraph many years ago. I might also

The President.

\* "Les Télégraphes," par A. L. Ternant (Paris, 1881), pp. 323-24.



say that the late Mr. C. V. Walker, one of our Past-Presidents, had a fusible wire in his lightning protectors. I should be glad if some one would open the discussion.

Sir David  
Salomons.

Sir DAVID SALOMONS: As this subject may lead to a long discussion, would it not be well to adjourn the meeting?

The  
President.

The PRESIDENT: I think it would be well to adjourn the discussion until the meeting after the inaugural address of next year.

Mr.  
Siemens.

Mr. ALEXANDER SIEMENS: There might be an extra meeting a fortnight after the President's address, upon which, of course, there will be no discussion.

The  
President.

The PRESIDENT: I should not like to say anything about that, because the next President has yet to take his seat.

Mr. Preece

Mr. W. H. PREECE: I might say this, Sir—that I know there are about me here several gentlemen who are very anxious to speak on this subject. I am particularly anxious to speak myself. I shall probably occupy a quarter of an hour or twenty minutes, or perhaps more; and as there are several who are equally anxious to speak, I think we ought certainly to have the subject adjourned; and inasmuch as next Thursday is fixed for the Soirée which you are kind enough to give us, it is impossible that we can hold another meeting this year. But the subject will grow. Mr. Cockburn has given us a great deal of food for thought. There are a good many here who may pursue the subject, and therefore if, after the President's address, on the earliest occasion, we have the question reopened, either by Mr. Cockburn again, or by somebody else, who will give us a short paper, then I think we can give one or two evenings to the discussion of what is one of the most important subjects connected with electric lighting and its progress. Therefore I propose that the discussion be adjourned now, and that the whole question be reopened on the earliest day after the new President's address next year.

The  
President.

The PRESIDENT: I suppose we ought to say that the proposed adjournment is arranged subject to the wishes of the new President. The opinion seems to be unanimous, and we will suggest to our new President at a meeting of the Council that

perhaps he will take the discussion upon Mr. Cockburn's paper at a meeting after his address.

Professor S. P. THOMPSON: There is one argument that might be adduced for taking the discussion upon this subject as early as possible, which is that this question of safety fuses somewhat affects the matters which are at present under the cognisance of the committee dealing with Fire Rules. I think it is most important that this discussion should take place before the report of that committee is made. It would be nothing short of a disaster if they were to make their report first, because two sets of rules would be set up which would puzzle those desirous of setting up good installations. I make these remarks to show that it is important, to avoid the confusion of having two sets of rules, not to adjourn this discussion till too late.

Mr. W. H. PREECE: As chairman of that committee, I may say that there is not the slightest prospect or likelihood of the committee altering any rule so as, or of doing anything that is likely, to create confusion of any sort or kind.

The PRESIDENT: Then I think we may consider the proposition carried.

A hearty vote of thanks was accorded to Mr. Cockburn for his very interesting and valuable paper.

The Scrutineers handed in their report of the result of the ballot for Council and Officers, which the PRESIDENT declared to be as follows:—

*President:*

EDWARD GRAVES.

*Vice-Presidents:*

J. HOPKINSON, M.A., D.Sc., F.R.S.	Professor W. E. AYRTON, F.R.S.
WM. CROOKES, F.R.S., Pres. C.S.	ALEXANDER SIEMENS.

*Ordinary Members of Council :*

E. B. BRIGHT, M. Inst. C.E.	GISBERT KAPP, Assoc. M. Inst. C.E.
Captain PHILIP CARDEW, R.E.	
R. E. CROMPTON, M. Inst. C.E.	H. R. KEMPE, Assoc. M. Inst. C.E.
Prof. J. A. FLEMING, M.A., D.Sc.	Professor JOHN PERRY, M.E., D.Sc., F.R.S.
Professor GEORGE FORBES, M.A., F.R.S.S. (L. & E.)	Prof. A. W. RÜCKER, M.A., F.R.S.
Captain Sir DOUGLAS GALTON, K.C.B., D.C.L., F.R.S.	Sir DAVID SALOMONS, Bart., M.A.
	AUGUSTUS STROH.

*Associate Members of Council :*

W. T. GOOLDEN, M.A.		W. M. MORDEY.
Major M. T. SALE, R.E., C.M.G.		

*Honorary Treasurer :*

EDWARD GRAVES.

*Honorary Auditors :*

J. WAGSTAFF BLUNDELL (Wagstaff Blundell & Co., Chartered Accountants, 12, Delahay Street, Westminster).  
FREDERICK C. DANVERS, India Office, S.W.

*Honorary Solicitors :*

Messrs. WILSON, BRISTOWS, & CARPMAEL, 1, Copthall Buildings, E.C.

The PRESIDENT: We are much obliged to the Scrutineers for their services this evening, and I beg to move a hearty vote of thanks to them for having performed that duty.

The motion was carried unanimously.

A ballot took place, at which the following candidate was elected :—

*Student :*

Charles Priest.

Captain Sankey, R.E., exhibited the Ordnance Survey Jubilee Book, being practically a duplicate of the one presented to Her Majesty the Queen, by the officers and men of the Department, descriptive of the work done on the Ordnance Survey during the last fifty years ; with specimens of the various maps, photo-zincograph copies of ancient manuscripts, and electrotype copper plates. The binding was formed of copper plates, richly ornamented with inlaid silver, produced by a photo-electrotype process ; the whole being the work of the Department.

The thanks of the meeting were unanimously accorded to Captain Sankey for his kindness in exhibiting this interesting memorial.

The meeting then adjourned.

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